This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

+ *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.

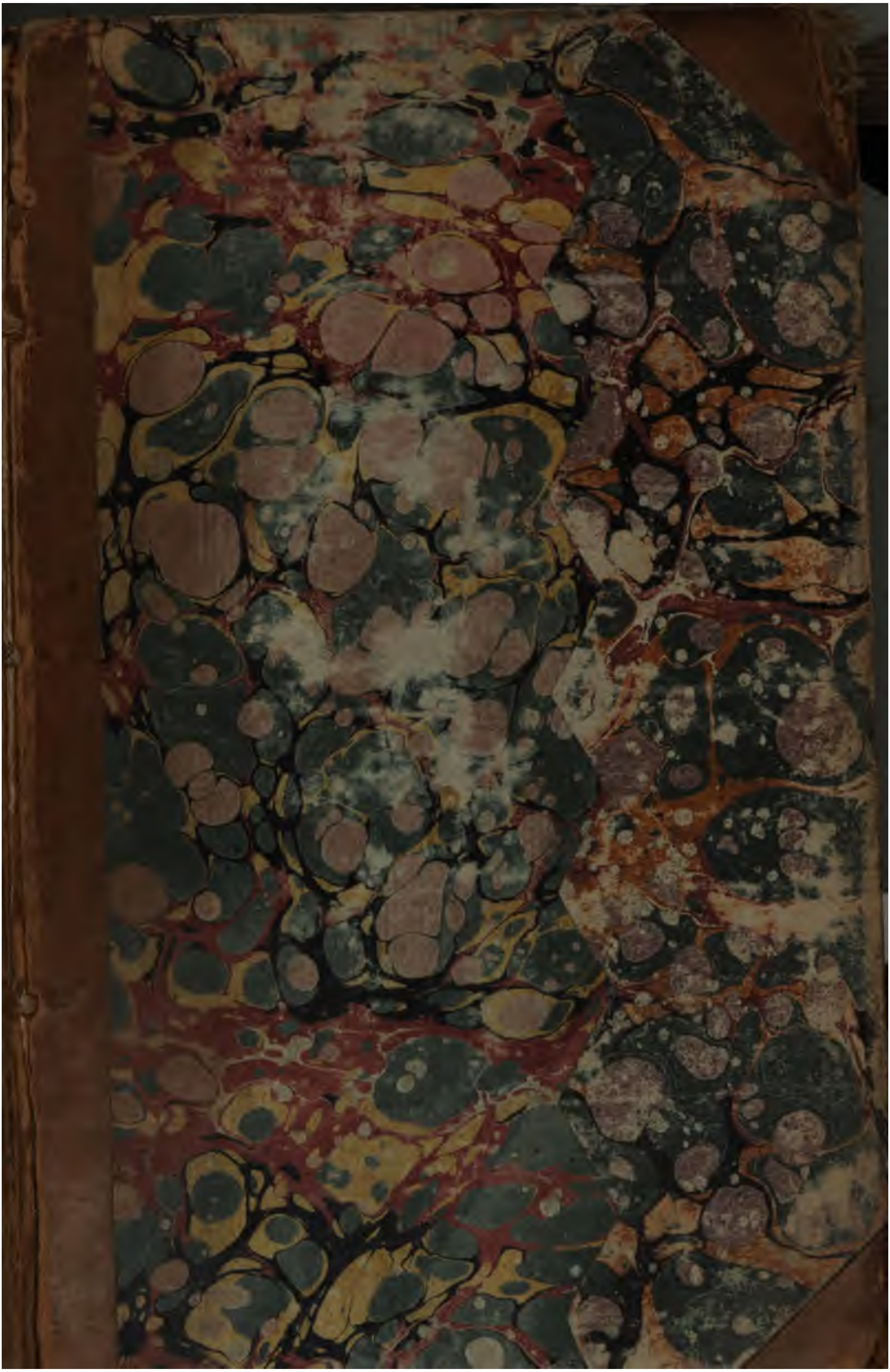
+ *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.

+ *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.

+ *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at `http://books.google.com/`



600038291T

E. BIBL. RADCL.

7. 4 2. H. 3/5 3
10.

1996 e 439/2

THE PHILOSOPHICAL

HISTORY

AND

MEMOIRS

OF THE

Royal Academy of Sciences at *Paris*:

OR,

An ABRIDGMENT of all the PAPERS relating to *Natural Philoſophy*, which have been publiſh'd by the *Members* of that *Illuſtrious Society*.

With many Curious OBSERVATIONS relating to the Natural Hiſtory and Anatomy of Animals, *&c.*

Illuſtrated with COPPER-PLATES.

The Whole Tranſlated and Abridged,
By *JOHN MARTYN*, F.R.S.
Profeſſor of Botany in the Univerſity of *Cambridge*;
AND
EPHRAIM CHAMBERS, F.R.S.
Author of the Univerſal Dictionary of Arts and Sciences.

VOL. II.

LONDON:
Printed for JOHN and PAUL KNAPTON, in *Ludgate-ſtreet*; and FRANCIS COGAN, and JOHN NOURSE, near *Temple-Bar*. M.DCC.XLII.

A
TABLE
OF THE
PAPERS contained in the Abridgment of the History and Memoirs of the Royal Academy of Sciences at Paris, for the Year Mdcciii.

In the HISTORY.

I. *ON the ways which ſeveral bodies turn.*
II. *Of a little inſect, called* puceron.
III. *Of the yellow amber.*
IV. *Of a valuable ſort of alabaſter.*
V. *Of a bottle of water kept for a great many years.*
VI. *Of a ſurpriſing recovery from deafneſs and dumbneſs.*
VII. *Of a ſtorm of hail of an extraordinary ſize.*
VIII. *Of a magnetical experiment.*
IX. *Of the effect of vinegar on ſome ſtones.*
X. *The former account of the inacceſſible mountain found to be falſe.*
XI. *Another fiction of an uncommon ſhining of the ſea.*
XII. *Of ſome petrified bodies found incloſed in ſtones.*
XIII. *Why the bottom of a pot is not ſo hot, when the water contained in it boils, as when it does not boil?*
XIV. *That the heat of water does not increaſe after it has boiled.*
XV. *On a monſtrous lamb.*
XVI. *Of the oriental and occidental bezoar.*
XVII. *Of a lizard from the* Eaſt-Indies.
XVIII. *Of a puppy, which had only one eye in the middle of his face.*
XIX. *On the valves of pumps.*
XX. *On the recoil of fire-arms.*
XXI. *On the force of machines in general.*
XXII. *On frictions.*
XXIII. *On the courſe held by ſeveral bodies tied together by cords, and drawn on a horizontal plane.*

In the MEMOIRS.

I. *Obſervations on the quantity of rain, that fell at* Paris, *at the royal obſervatory, on the thermometer, and on the barometer, during the year* 1702, *by M.* de la Hire.

II. *The thermometer reduced to a fixed and certain meaſure, and the means of referring to it the obſervations made with the old thermometers, by M.* Amontons.

III. *Remarks on rain water, and on the origin of ſprings, with ſome particularities concerning the conſtruction of ciſterns, by M.* de la Hire.

IV. *That the new experiments on the weight and elaſticity of the air ſhew, that a moderate degree of heat may reduce the air into a ſufficiently violent ſtate, to be the ſole cauſe of very great earthquakes and commotions on the terreſtrial globe, by M.* Amontons.

V. *Remarks on the table of the degrees of heat, extracted from the* philoſophical tranſactions *for* April 1701; *read by M.* Geoffroy *in the aſſembly, on* Tueſday, July 24, 1703, *by M.* Amontons.

VI. *Barometrical experiments made upon ſeveral mountains of* France, *by M.* Maraldi.

VII. *Obſervations on a petrified brain, by M.* du Verney, *junior.*

VIII. *Remarks on the inequalities of the motion of pendulum clocks, by M.* de la Hire.

IX. *A way to raiſe a great veſſel upon the hold, as it is conſtructed in the port of* Toulon, *without making uſe of any machines, by M.* de la Hire.

X. *A deſcription of the heart of a ſea-tortoiſe, by M.* Mery.

XI. *A deſcription of a great* American *land-tortoiſe, with reflections upon that of M.* du Verney, *by M.* Mery.

AN ABRIDGMENT OF THE Philoſophical Diſcoveries AND OBSERVATIONS IN THE HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1703.

I. *On the ways which ſeveral bodies turn; tranſlated by Mr.* Chambers.

'TIS not only underſtanding that we want for the diſcovery of natural cauſes, but we ſeem alſo to want eyes for the perceiving of effects, and a thouſand things are continually preſenting themſelves, which we never ſee.

How few people have obſerved, that the ſhells of ſnails, which are ſpirals wound about a kind of cone, are all wound the ſame way in the ſame ſpecies; and what is more, the ſame way in almoſt all ſpecies is from left to right, conſidering them with the point upwards. M. *Parent*, after ſeveral enquiries among all the land, river, ſea, and even ſtone ſnails, has only found three kinds, whoſe ſhells are turned from right to left.

The ſame obſervation he has extended to plants. There are a multitude of plants, which have either their ſtems, flowers, or pods twiſted a certain way: others, whoſe flowers, or ſeeds, are faſtened ſpirally around the ſtem; and conſequently are faſtened thereto in a determinate way. Now this way is always the ſame in the ſame ſpecies; and what is more, M. *Parent*, having made the moſt complete enumeration of the ſeveral kinds of plants, with reſpect to ſuch properties, has always found, that one certain way prevailed; that is, for inſtance, that for 33 ſpecies of plants, whoſe ſtems were twiſted to the right, there were only 4 which had untwiſted the other ways; and that for 15 ſpecies of pods twiſted to the left, there were only 2 to the right.

M. *Parent* has likewiſe obſerved, that the fibres of the human heart are always twiſted the ſame way; the external ones from right to left deſcending, and the internal ones the ſame way aſcending; and that on the contrary, the little tuft, formed by the young hairs on the top of the head, is almoſt always twiſted from left to right, with regard to the perſon it is on.

It may ſuffice to have hinted, that there are obſervations to be made upon ſuch kinds of ſubjects, commonly ſo little obſerved; and that a man may turn his eyes and attention that way, every body hereafter will find the like occaſions enough in nature.

The conſtant and invariable determination in ſome parts, whether of a plant, or an animal, to be twiſted the ſame way, does doubtleſs ariſe from the ſeed or the egg; and this is a new proof, that generation is only unfolding; but who gave the egg or the ſeed this determination?

The ſame queſtion may be made, as to the way wherein the planets turn; and the whole may be referred to the arbitrary will of him who made the univerſe. Another queſtion, *viz.* why a greater number of plants, or ſhells, are twiſted a certain way muſt be ſolved from the concatenation of ſecond cauſes?

M. *Parent* conjectures, that this may have ſome dependance on the ſyſtem of the loadſtone. He is convinced, that we may admit two vortices of magnetical matter, which winds ſpirally around the earth, and along its axis, in two ways oppoſite to each other; one whereof iſſues at one hemiſphere, and the other at the oppoſite one. This magnetical matter is ſubtile enough to penetrate bodies, and conſequently the eggs or ſeeds of bodies; and if by any cauſe whatever, one of the vortices be more diſpoſed to penetrate certain ſeeds or eggs, than the other, it will twiſt them in its own way. Each vortex perhaps may commonly have moſt power in the hemiſphere it iſſues at, on which footing the bodies capable of being thus twiſted, ſhould effect a contrary way in the ſouthern hemiſphere from what they effect in ours; but this notion wants to be confirmed by obſervations.

II. *Of a little inſect, called* puceron.

M. *de la Hire* has obſerved the little inſects, called *pucerons*, becauſe they are like little green [*puces*] fleas. They faſten themſelves to the young ſhoots of trees and plants, and deſtroy a part of them. The leaves under which they place themſelves, curl quite up, and ſerve them at the ſame time for a covering againſt the injuries of the air, and for nouriſhment. They hatch about the middle of ſpring, and grow very conſiderably in about a month. They have 3 feet on each ſide, and two pretty long horns on their heads. They are of a long rounded ſhape, the head ſmall, with 2 eyes of a reddiſh brown. Some time afterwards, they are ſeen with wings. M. *de la Hire* has found on the plants, which they eat ſo great a quantity of white ſpoils, containing the feet and horns of this animal, that he does not queſtion their being coverings, which the *pucerons* quit, which have their horns and legs faſtened to them, and hide the little wings, which they muſt uſe, after the legs are fallen off. Theſe wings entirely diſengage themſelves in half an hour; they are then very white, but after they are diſengaged, they grow black by degrees, and perfectly like thoſe of the common flies. The head of the inſect becomes black alſo, the body brown, and it diminiſhes a little in bigneſs.

Here then is a ſpecies of inſects, which, from walking, come to fly, without paſſing as moſt other flying inſects do, thro' the ſtate of an *aurelia*, or *chryſalis*. Inſtead of paſſing thro' this metamorphoſis, theſe *pucerons* quit their covering; it perfectly reſembles that with which the frogs are cloathed, when they are in the ſtate of tadpoles, incapable of walking on the ground, and fit only to ſwim.

M. *de la Hire* thinks the *pucerons* live a whole year, and that, during the winter, they retire into holes, whence they come out in the ſpring to lay their eggs, as the common flies do.

The ants are very fond of the *pucerons*. They gather in great quantities on the plants where they are, and the diſeaſes of theſe plants are imputed to them; but they are *pucerons* that have done all the harm; and the ants have only drawn them thither.

III. *Of the yellow amber.*

M. *Galland*, of the academy of inſcriptions, has confirmed the academy of ſciences in what was ſaid of the yellow amber, in the hiſtory of 1700 *. He found ſome of it at *Marſeilles*, on the ſea ſhore, in a place where there were no trees, and where the ſea was bounded only by very ſteep rocks, which were beaten by the waves in rough weather. The yellow amber muſt have been detached from the clefts of theſe rocks, whence it fell into the ſea.

IV. *Of a valuable ſort of alabaſter.*

At the ſame time M. *Galland* informed the academy, that he had ſeen at the ſeat of the illuſtrious M. *Puget*, of *Marſeilles*, ſome columns of alabaſter of different colours, and very precious. It is ſo tranſparent, that thro' the very perfect poliſh of which it is capable, one may ſee above two fingers into its thickneſs the agreeable variety of colours with which it is embelliſhed. M. *Puget* told M. *Galland*, that he was the only perſon who knew the quarry, tho' it was not far from *Marſeilles*.

V. *Of a bottle of water kept for a great many years.*

M. *Dodart* ſhewed a bottle of water of *Sainte Reine*, kept ever ſince the year 1678, without any corruption, or any ſediment that appeared at the bottom. There was however a little air in the bottle. The wicker of it was rotten. Upon breaking it, there was found at the bottom a light earthy ſediment, which had nothing ſaline in it. Some only were of opinion, that this earth might be a little tartarous.

---

* See Vol. I. *pag.* 186 of this abridgment.

VI. *Of a ſurpriſing recovery from deafneſs and dumbneſs.*

M. *Felibien*, of the academy of inſcriptions, informed the academy of ſciences of a ſingular and perhaps unheard of accident, which happened at *Chartres*. A young man of 23 or 24 years of age, ſon of an artiſan, who had been deaf and dumb from his birth, began all at once to ſpeak, to the great aſtoniſhment of the neighbourhood. They found that 3 or 4 months before, he had heard the ſound of bells, and been extremely ſurpriſed with this new and unknown ſenſation. Afterwards there came a ſort of water out of his left ear, and he heard perfectly with both ears. He ſpent 3 or 4 months in hearing, without ſpeaking at all, accuſtoming himſelf to repeat low the words which he heard, and confirming himſelf in the pronunciation, and in the ideas annexed to words. At laſt, he thought himſelf fit to break ſilence, and declared that he ſpoke, tho' it was yet but imperfectly. Some able divines immediately aſked him about his paſt ſtate, and their principal queſtions turned upon God, the ſoul, and the moral goodneſs or evil of actions. He did not ſeem to have carried his thoughts ſo far. Tho' he was born of catholic parents, went to maſs, was inſtructed to make the ſign of the croſs, and to kneel in a poſture of praying; yet he never added any intention to all this, nor comprehended what others intended thereby. He did not know diſtinctly what death was, and had never thought about it. He led a mere animal life, entirely engaged upon ſenſible and preſent objects, and on the few ideas which he received by his eyes. He did not even draw from the compariſon of theſe ideas all, that one would have thought, he might draw from it. Not that he had naturally any want of underſtanding, but the underſtanding of a man deprived of the converſation of others, is ſo little exerciſed, and ſo little cultivated, that he does not think ſo much thereon, as he is indiſpenſably obliged by external objects. The greateſt fund of the ideas of men is in their reciprocal converſation.

VII. *Of a ſtorm of hail of an extraordinary bigneſs.*

M. *Parent* related, that *May* 15, there fell about *Iliers*, in *le Perche*, a prodigious quantity of *hail*; which was alſo of a prodigious bigneſs. The leaſt was as big as one's two thumbs, and the biggeſt were of the ſize of one's fiſt, and weighed five quarters of a pound, and the middle ſize was like pullets eggs, and in much greater quantity. It fell in ſeveral places a foot deep. There were 30 pariſhes, in which the corn was cut as it were with a ſcythe. The inhabitants of *Iliers*, ſeeing this ravage, had recourſe to their bells, which they rung ſo briskly, that the cloud was parted over their pariſh into two parts, and ſeparated in ſuch a manner, that their pariſh alone, in the midſt of 30 others, which had not ſuch good bells, was very little injured. M. *Parent* related farther, that as the corn was then not very forward, tho' for the moſt part in the ear, it made new ſhoots from the bottom, and that theſe ſtalks began to form little ſpikes, which they hoped would come to maturity; we have ſince heard that they had a good harveſt.

VIII. *A magnetical experiment.*

A plate of ſteel touched with a loadſtone, ſuſtains a greater weight when it is longer. M. *Deſcartes*, and after him M. *Rohaut*, have thought, that this increaſe of force came from the magnetical matter acquiring more quickneſs in paſſing thro' a longer plate, becauſe it finds the ways more eaſy than otherwiſe. M. *Joblot*, who has ſtudied the loadſtone particularly, and made a great many diſcoveries on this ſubject, in a diſcourſe one day with M. *Carré*, ſuppoſing this augmentation of the quickneſs of the magnetical matter, found him not much diſpoſed to embrace this opinion. As M. *Carré* was diſputing againſt it, there came an experiment into his mind, which would clear up the truth. It was to make three plates of good ſteel, well poliſhed, an inch broad or thereabouts, one of which ſhould be twice the length of the other two, and of the ſame weight with both of them together, to draw them an equal number of times on the loadſtone, that they may be touched as equally as poſſible, and to ſee afterwards what weight the longeſt would ſuſtain alone, and the two ſmall ones put one upon another, ſo that the poles of the ſame name might be together, for otherwiſe they would have no effect. If the increaſe of force of one longer plate came from the magnetic matter, having increaſed its quickneſs in paſſing thro' it, then the long plate muſt ſuſtain a greater weight than the two ſmall ones; but if this augmentation of force came, as M. *Carré* thought, only from a greater quantity of magnetic matter paſſing thro' a longer plate, then the two ſmall ones muſt ſuſtain as much as the great one. The experiment was made by M. *Joblot*, and the longeſt plate ſuſtained eight ounces two drams; and the two ſmalleſt, a little more than ſeven ounces; their not going beyond eight ounces two drams was occaſioned by their not being wrought enough to join exactly; for we know that the more two plates are united, the more force they have.

To this experiment was added a fourth plate, equal in length to the two ſmall ones, but a little heavier than the great one. It had been touched like the other three, and it ſuſtained only a dram more than each of the ſmall ones, which came probably from its thickneſs, which had rendered it more hard to be penetrated by the magnetic matter. Hence it follows, that among plates of equal thickneſs, and conſequently equally penetrable by the magnetic matter, the longeſt have the greateſt force, only becauſe a greater quantity of this matter has entered into them.

Other experiments, that were made with the ſame deſign on the ſame plates, terminated in the ſame concluſion.

IX. *Of the effect of vinegar on ſome ſtones.*

M. *Carré* has related, that having gathered ſome little flat ſmooth ſtones among the ſand in the river; he put them into a baſon, the bottom of which was a little inclining from the ſides to the centre, and placed them at the circumference; that upon pouring in ſome vinegar, all the little ſtones got preſently to the centre. He thinks the reaſon of this effect to be, that the vinegar cauſes a diſſolution in the ſtones, and conſequently drives out the air which is then under them, raiſes them up, and makes them roll on an inclined plane. For the ſame reaſon the ſtar-ſtone turns in diſtilled vinegar, and on a horizontal plane.

X. *The former account of the inacceſſible mountain found to be falſe.*

It was ſaid in the hiſt. of 1700 *, that if the inacceſſible mountain of *Dauphiny*, which has its point at bottom and baſe at top, was well obſerved, it might be found to be a miſtake. This has really been the caſe, the academy has been informed both by M. *de Vaubonnays*, firſt preſident of the chamber of accounts of *Grenoble*, who is pleaſed to be their correſpondent, and by a letter from M. *Caſſet*, ſecretary to M. *Bouchu*, intendant of *Dauphiny*, to M. *de la Hire*, that this pretended inacceſſible mountain, which is eight or nine leagues from *Grenoble* to the S. is nothing but a ſteep rock planted on the top of an ordinary mountain, and that even this rock has not the figure of an inverted pyramid. Beſides, there is no appearance of any rock or part of a rock having changed the figure, which this marvellous mountain might have in the time of *Charles* VIII. for it is between ſome mountains of a living rock, and we do not find at the bottom any ruins of rocks, as we do in many other places.

---

* See Vol. I. pag. 181 of this abridgment.

XI.

XI. *Another fiction of an uncommon ſhining of the ſea.*

There came a letter from *Cadiz*, importing, that for 15 nights together they had ſeen the whole ſea ſhining with a clear light, almoſt like a liquid *Phoſphorus*; and to make this compariſon the more perfect, that the ſea water being carried away in bottles, gave the ſame light in the dark; that ſome drops of it being let fall on the ground ſhone like ſparks of fire, and that linnen dipt in this water became alſo luminous. The fact having been well examined, is found to be falſe. At moſt, this report, which was ſpread even in *Spain*, might be founded on ſome particular and lively colour, which the ſea might have at ſunſetting. The academy think, they do as much ſervice to the publick, in diſabuſing them with regard to falſe wonders, as in recounting to them the true.

XII. *Of ſome petrified bodies found incloſed in ſtones.*

M. *Maraldi* has brought from *Italy* ſome hard ſtones, of a whiteiſh colour, which ſplit into leaves, and diſcover dried fiſhes, chaff and olive leaves. They were found in the *Veroneſe*, by the chevalier *Bianchi*. It has happened luckily, that in cleaving the greateſt part of thoſe which contained a petrified fiſh, it has been ſplit in the middle of its thickneſs, in ſuch a manner that the two parts are very eaſy to be known. They ſeem as if imprinted in a mould. All the outer part of the body of the animal is very exactly marked, and there is no room to queſtion their being real fiſhes, involved in a ſand which afterwards petrified.

M. *Maraldi* ſaw, in the grand duke's gallery, fiſhes of the ſame kind dried in ſtones, which had been taken in *Phœnicia*, in the territory of *Biblis*, now called *Gibeal*, on ſome mountains almoſt inacceſſible, and 15 miles diſtant from the ſea.

He ſaw alſo at *Rome* ſeveral large pieces of marble, mixt with red and white, which come from the mountains of *Sicily*. The white part of them is formed of a great quantity of ſhells incloſed and incorporated in the marble.

What can have brought theſe fiſhes and ſhells into the earth, and even to the tops of mountains? Probably there are ſubterraneous fiſhes, as well as ſubterraneous waters; and theſe waters, which, according to M. *de la Hire*'s ſyſtem, riſe in vapours, carry with them perhaps ſome very light eggs and ſeeds, after which, when they are condenſed and replaced in the water, theſe eggs may hatch, and become fiſhes or ſhells. And if theſe currents of water already raiſed a good deal above the level of the ſea, and perhaps even to the tops of mountains, come by ſome accident either to dry up, or to take another courſe among ſands, and at laſt abandon in any manner whatſoever the animals which were nouriſhed in it, they will be left dry, and involved in earth, which petrifying itſelf, will petrify them alſo. Theſe waters themſelves may be petrified, after having paſſed thro' certain earths, and be loaded with certain ſalts. If all ſtones have been liquid, as ſome great philoſophers think, then this ſyſtem is the more probable.

XIII.

XIII. *Why the bottom of a pot is not ſo hot, when the water contained in it boils, as when it does not boil.*

One day a queſtion was aſked, why a veſſel full of boiling water is leſs hot at the bottom, whilſt the water being taken off the fire continues to boil, than when it ceaſes to boil; for whilſt the water continues to boil, one may touch the bottom of the veſſel with one's hand, without burning it, which one cannot do as ſoon as it has done boiling. We muſt add, that to make this experiment ſucceed, the bottom of the veſſel muſt be thin, and the veſſel itſelf pretty large.

M. *Homberg* ſaid, that he conceived a body was hot, only becauſe it was penetrated every way by the matter of flame or of light, which coming out of all parts with impetuoſity, like an infinite number of very ſharp little darts, carried the impreſſions of the heat into all the bodies that it ſtruck; that when a veſſel is on the fire, the flame being driven upwards by the weight of the air, endeavoured to make paſſages for itſelf into the water of the veſſel; according to this direction, that at firſt it muſt have found it difficult to penetrate this water, where there were no ſuch paſſages as its motion required, that it made them at laſt, and in this ſtate the water boiled, that then all the paſſages upwards thro' the water were eaſy, the flame, which inceſſantly ſtruck the bottom of the veſſel, drove them eaſily thro', that conſequently the veſſel being taken off the fire, the motion of all the little darts was at that inſtant upwards, and ſo they could not hurt the hand which touched the veſſel; but that when it had done boiling, all its parts being leſs agitated, ſinking and falling one upon another, cloſed an infinite number of paſſages, that before were open, which obliged the little darts to eſcape indifferently on all ſides, and that conſequently the hand being applied to the bottom of the veſſel, muſt receive a great number and be hurt by them.

The bottom of the veſſel is regarded here only as a mere ſurface, which the flame paſſes thro' only before it arrives at the water, and on which it does not act. But if this bottom has any thing of a conſiderable thickneſs, the flame neceſſarily acts upon it, as it penetrates, not only upwards, according to the direction impreſſed by the weight of the air, but alſo downwards, and according to all the determinations, becauſe it is reflected by the ſolid parts; and thence it comes, that in this caſe, independantly on the different ſtates of the water, the hand will always perceive heat in touching the bottom. If it is thin, and at the ſame time the veſſel is very ſmall, then the ſides of the veſſel, which almoſt encompaſs the bottom, communicate their heat to it, and conſequently the veſſel can no more be too big, than the bottom can be too thin.

XIV. *That the heat of water does not increaſe, after it has boiled.*

This little ſyſtem of heat has ſupplied M. *Homberg* with the reaſon of a ſurpriſing fact, which M. *Amontons* has diſcover'd in working at his thermometer, and has ſerved him in the conſtruction of it. If water boils as long as it can, its degree of heat does not increaſe, tho' it is kept longer on the fire, or on a greater fire. Water, according to M. *Homberg*, is hot only becauſe it is penetrated by the matter of the flame, which ſwells, raiſes, and roughens its ſurface;

when this matter has made free paſſages on all ſides thro' the water, the water boils as much as it can, and the matter of the flame can do nothing farther; therefore, whether there is a continual ſucceſſion of new matter, or whether it comes in greater abundance, it neither can open the paſſages better, nor open a greater quantity of them.

XV. *Of a monſtrous lamb.*

M. *Antoine*, ſurgeon at *Mery* upon the *Seine*, a correſpondent in anatomy with M. *Mery*, having met with a very remarkable monſter, new conſequences were drawn from it, with regard to the circulation between the mother and the *fœtus*, which were communicated to M. *Mery*, and by him to the academy.

A ſheep brought forth two lambs, one of which was alive, well formed in all its parts, at its full time, and the other dead and monſtrous, without either head, breaſt, *vertebræ*, or tail, having only a ſort of belly, at the end of which were the hinder thighs, legs, and feet. This belly was round and oblong, being ſeven or eight inches in its greateſt length, five or ſix in its greateſt breadth, and above three inches in thickneſs; it had no aperture, nor any diſtinction of ſex; it was very fleſhy, as well as the thighs, covered with a skin juſt like that of young lambs. In the middle of the fore-part was the navel-ſtring, compoſed of four pretty thick veſſels, and very well diſtinguiſhed.

The inſide of this deformed trunk was the moſt wonderful; it had neither heart, lungs, liver, ſpleen, kidnies, bladder, ſpermatic veſſels, nor any parts of generation, but only a meſentery and inteſtines, which were faſten'd to it, and a ſort of ſtomach, which however had no ſhape. This ſtomach and the inteſtines contained a yellow and viſcid liquor, like excrements.

The umbilical veſſels were the origin and end of all the reſt; and all the nerves began from a pyramidal body, of the bigneſs of a walnut, which conſequently ſupplied the place of the brain. The whole fabrick of the maſs, or the aſſemblage of the bones, conſiſted of a bone ſituated in the upper part of the monſter, of two *oſſa ilia*, or *innominata*, which formed a ſort of baſon, and of the bones of the lower extremities. This pyramidal body, which repreſented the brain, was faſtened to the bone of the upper part.

Two animals faſtened together by ſome common part, or a ſingle animal with thoſe parts double, which are naturally ſingle, would be much leſs ſurpriſing than this half animal; and therefore are not ſo rare. Two *ova*, meeting in the *uterus*, may ſtick together, and yet receive nouriſhment, and then, either all the parts of both unfold and ſubſiſt, and they are two animals faſtened together; or elſe ſome parts of one of them periſh, and then remains an animal and a half animal united, in ſuch a manner, that the half animal finds all that it wants in the whole animal. But as for a half animal, which does not join to another, but is deprived of all the moſt neceſſary parts, as the heart, lungs, and liver; how could ſuch a one be formed and nouriſhed? how could nature detach a half from a whole, that is ſo well connected, and ſo indiviſible as an animal? and how could ſhe detach the moſt depending half from that which governs it, and contains the principal ſprings of the machine?

It neceſſarily follows from the want of the heart in this monſter, that the impulſe of the circulating blood did not proceed from the heart, and conſequently M. *Antoine* has ſought for another principle of it.

According to the common ſyſtem, the arteries of the *uterus* pour the blood into the *placenta*, which is nouriſhed thereby; the overplus of this blood enters into the umbilical veins, which make part of the navel-ſtring; thence it is carried to the liver of the *fætus* in the trunk of the *vena portæ*, whence it paſſes into the *vena cava*, and into the right ventricle of the heart. The blood of the mother being once arrived at the heart of the *fætus*, is afterwards uſually diſtributed into the parts of the *fætus*, except the alterations made in its courſe by the *foramen ovale*, and the canal of communication. There is yet another neceſſary difference, that the navel-ſtring and the *placenta* muſt be comprehended in the circulation; and for this reaſon, the blood, which comes out of the iliac arteries of the *fætus*, enters the navel-ſtring by the umbilical arteries in the *placenta*, where it is taken up again by the veins of this part, and afterwards by the umbilical veins, which carry it back with new blood from the mother, to the liver and heart of the *fætus*.

As the heart was wanting in M. *Antoine*'s monſtrous lamb, either the blood of the mother, carried into the *fætus* by the umbilical veins, according to the common ſyſtem, muſt have returned to the *placenta* by the umbilical arteries, contrary to the ordinary rules of the circulation, which do not ſeem to permit this return of the blood by the arteries, or elſe this very blood being driven by the mother into the arteries of the *placenta*, muſt have paſſed thence into the umbilical arteries, diſperſed itſelf into all the parts of the *fœtus*, and returned to the *placenta* by the umbilical veins; which ſeems more conformable to the laws of circulation, but entirely reverſes the common ſyſtem.

M. *Antoine* has choſen rather to give up this ſyſtem, than to admit an exception to the ordinary laws of circulation. He has even thought, that if in this monſter the blood of the mother entered by the umbilical arteries, it followed the ſame courſe in all the *fœtus's*, and he ſent a paper to the academy, wherein this new opinion was ingeniouſly explained and ſupported.

We muſt not forget to obſerve in the monſtrous lamb, the extraordinary brain, placed in a ſtill more extraordinary manner in the belly, which was the whole animal. We may thence conjecture, how much a common principle of the nerves is neceſſary and indiſpenſable in the whole animated machine.

XVI. *Of the oriental and occidental bezoar.*

The bezoar is nothing but the gall-ſtone of ſeveral ſorts of animals, both of the *Eaſt* and *Weſt-Indies*, as goats, hogs, monkies, *&c.* The virtue of the bezoar conſiſts in the volatile alkaline ſalts of which it is formed, ſince it is in effect nothing but the bile of theſe animals. It is thereby that it deſtroys acids, and promotes perſpiration, and without going ſo far to ſeek for bezoar, every gall-ſtone of an animal is ſuch, tho' perhaps more or leſs active, according to the different animals, and the different climates where they live. The bezoar ſometimes alſo gets a little from other parts beſides the gall.

XVII. *Of a lizard from the* Eaſt-Indies.

F. Gouye ſhewed a lizard from the *Eaſt-Indies*, called by the natives *Phatagen*, and by *Aldrovandus*, *Lacerta Indica Squammoſa*. It was two feet ½ long, ſhaped almoſt like a crocodile, covered with ſcales of the ſize of a crown-piece. His ſtomach was found full of ants, which are the common food of this animal; it has alſo a tongue near a foot long, to catch them the more eaſily. It had in the upper part of the ſtomach a bag full of living worms, about the thickneſs and length of pins, in number about 1000, and, which is very aſtoniſhing, it was no diſeaſe; there were as many found in another animal of the ſame *ſpecies*. We have already obſerved the ſame thing in a tiger from *China*, in the hiſtory of 1699 *.

XVIII. *Of a puppy, which had only one eye in the middle of its face.*

M. *Littre* ſhewed a dead puppy †, which had but one eye, without an eye-lid, ſituated in the middle of the lower part of the face, juſt as the *Cyclops* are painted. It had neither noſe, nor throat, nor any aperture to ſupply them. It was fat and well nouriſhed, and died ſoon after it was born; no doubt becauſe it could not take in any air. This *fœtus* could not have been nouriſhed any other way than by the navel-ſtring. It had above its eye, and towards the middle of its face, a ſort of cartilaginous body, ½ an inch long, and a line thick, ſolid, of a conical figure, and without any aperture.

---

* Vol. I. Pag. 21. of this Abridgment.

† Plate I. Fig. 1.

XIX. *On the valves of pumps.*

M. *Amontons* having conſtructed a forcing pump, was ſurprized to ſee that the valves, which were of metal, well made, and well ordered, ſometimes ceaſed to play, and ſtopped on a ſudden. He thought ſome viſcid ſediment had ſtuck theſe valves together, but he found nothing like it, upon examining the pumps ſeveral times. The wonder of the effect conſiſted in this, that, when the piſton was raiſed, there was a *vacuum* formed between the piſton and the valve; that conſequently this valve was driven upwards, by the water, which was driven itſelf by the weight of the whole atmoſphere; that the body of the pump being immerged above ſix feet in water, the weight alſo of ſix feet of water preſſed the valve, being equal to about ⅕ of the weight of the atmoſphere; and yet that this valve, which was intended to open by this impulſe, did not open at all.

This effect may be compared to the known experiment of two ſurfaces well poliſhed and wetted, applied againſt each other, which cannot be ſeparated but by the action of a great weight; for the ſurfaces of the valves are in like manner applied to their ſhells as exactly as poſſible, and both of them are neceſſarily moiſtened. But there appears at firſt ſight an eſſential difference, that in the common experiment we may aſcribe the union of the two ſurfaces, or the difficulty of ſeparating them, to the preſſure of the air, becauſe,

cauſe, as there is no air between them, the whole atmoſphere reſiſts their ſeparation; on the contrary, with regard to the valves, it is the whole atmoſphere; and alſo a force ſtill greater, that tends to ſeparate them from their ſhells.

It remains therefore, that the whole force of the union of the valves and ſhells conſiſts in the water that moiſtens them; the parts of water, which have entered the pores of one of the bodies, muſt powerfully faſten upon the other, and every one of them muſt hold to the two bodies by its two extremities; for free and flowing parts of water would hinder the union, they muſt faſten ſo much the more powerfully, as the two ſurfaces are more poliſhed, better wrought, and exclude the air more perfectly; the multitude of the faſtened particles of air muſt contribute to the greatneſs of the effect; and in the laſt place, the difficulty either of detaching or extending them, muſt produce that of opening the valves.

It is certain, that to detach from the copper the parts of water which moiſten it, there muſt be a pretty conſiderable effort, and it muſt be only by the evaporation, or by a violent friction, and at ſeveral eſſays, that we can entirely obtain the effect. As for the extenſion of the parts of water, neither M. *Amontons*, nor all the reſt who have been willing to make the experiment, have been able to affirm, that water is capable of extenſion. Thus we may believe, it is all reduced to the difficulty of detaching the parts of water. It is more than probable, that they do not faſten with the ſame force to all ſorts of bodies.

It follows from the obſervation and reaſoning of M. *Amontons*, that in the experiment of the two ſurfaces, wetted and applied to each other in the open air, it is not only the preſſure of the at-

atmoſphere that makes their union, but the parts of water which wet them, have more or leſs ſhare therein, according as they are more or leſs ſtrongly faſtened to the different ſurfaces of the bodies; and this is a cauſe which we did not think of in the explication of this *phænomenon*, tho' it was at leaſt as important and efficacious, as that which was at firſt diſcovered, and it may act alone, or even combated by the other.

M. *Amontons* would have been glad to compare the force of the union produced by the parts of water, with that which is produced by the preſſure of the atmoſphere. The way was to immerge the pump deeper and deeper in the water, till at laſt the valves would open without difficulty; for we ſhould have ſeen thereby what weight was exactly required to overcome all the reſiſtance cauſed by the adhering of the parts of water, but this could not be put in practice, becauſe of the particular circumſtances of the place.

Theſe obſervations on the mutual adhering of bodies, and on the forces which produce it, may ſerve one day to diſcover the general cauſe of hardneſs, which is none of the leaſt queſtions in phyſicks; but whilſt we are expecting ſo elevated a theory, M. *Amontons* drew from his experiment ſomething uſeful in practice. He changed the valves of metal for flaps of leather, after which the pumps played perfectly well, becauſe leather not being near ſo ſolid as metal, the water does not adhere in the ſame manner. The inconvenience of the valves came from their being too well made, too well poliſhed, and too well fitted upon their ſhells, and certainly we ſhould never have expected, that too great a perfection would have rendered them uſeleſs. To prevent this accident, M. *Amontons* adviſes us to prefer leathern flaps.

XX. *On the recoil of fire-arms.*

M. *Geoffroy* having reported from the *philoſophical tranſactions*, that it had been found in the *royal ſociety* of *London*, that a certain charge of a fire-arm turned the ball from right to left, whilſt the cannon in recoiling went from left to right; this fact, tho' abſolutely poſſible, ſeemed very doubtful; and we were willing to be ſure of it, before we ſought for ingenious reaſons, which we might perhaps have had the misfortune to find.

M. *Caſſini* the ſon undertook the experiment, and he cauſed a machine to be made as like as poſſible to the *Engliſh* one. It was a wooden *Iſoſceles* triangle, upon which was placed and made very faſt a gun, of about 3 feet 8 inches; the breech of it reſted upon the baſe of the triangle, and the end upon the *vertex*. We muſt ſuppoſe, without entering into a more exact deſcription, that the machine was ſuſtained at its 3 angles upon 3 pivots, which hindered it from rubbing againſt the ground, that it might be rendered fixt and unmoveable by ſcrews, which entered into its pivots, and that ſo by taking away the ſcrews of the angle of the top, and of one of the angles of the baſe, it might turn upon the pivot of the third angle, as upon a centre, and that it turned very eaſily, becauſe there were rollers at the 2 angles, which were deſired to be moveable.

This being done, M. *Caſſini* the ſon faſtened the machine, charged his gun, fired it, obſerved the place where the ball ſtruck upon a board placed at 17 feet diſtance, then he took away the ſcrews from the angle of the *vertex*, and from one of the angles of the baſe, charged it again, and fired it.

The machine muſt then recoil, becauſe it was at liberty, and its recoil was the arch of a circle, which the moveable angle of the baſe deſcribed. The place was obſerved, where the ball had ſtruck, the machine being at liberty, and it was compared with that of the *fixed* ſtroke.

The experiment was repeated a great number of times, and with ſeveral different charges, as well for the fixed ſtrokes as for the others. As the machine always recoiled, or turned, when it was free, theſe ſtrokes did not always hit the ſame place with the fixed ſtroke, but they always went to the right of the fixed ſtroke, becauſe this circular recoil was made that way, and there never was found between the ſtroke and the recoil the contrariety of direction marked in the *Engliſh* experiment.

An advantage, which that of M. *Caſſini* had, is, that every time he replaced his fixed machine, he was well aſſured, that it was found again in the ſame ſituation, in the ſame direction, and that it had received no alteration from the preceding ſhakes; for all the fixed ſtrokes went always into the ſame hole. Perhaps, on the contrary, in the *Engliſh* experiment, the gun, which had not been held faſt enough, turned from its firſt direction; and this might be ſufficient to make the ſtroke have a contrary one to the recoil.

The recoil was always ſo much the greater, as the charge was ſtronger; but the ſtrokes of the machine at liberty were not diſtant from the fixed points, in proportion to the greatneſs of the recoil. On the contrary, the ſtronger the charge was, and the greater the recoil, the leſs did the free ſtrokes vary. This comes from the ball always going out with more velocity, there is leſs impreſſion felt of the recoil, which does not begin to ſhake the machine, till the ball is upon the point of going out.

XXI. *On the force of machines in general.*

The multiplication of forces by machines has ſomething deceitful in it, not with regard to thoſe who know the principles of mechanicks, but to the general part of the world, on whom the engineers may impoſe by magnificent promiſes; and ſometimes to the engineers themſelves, if they have more invention or boldneſs than knowledge.

The academy ſometimes hears of ſome of theſe engineers and their lofty propoſals. One man is to have as much effect as 50, to raiſe a burden, and when it is objected to them, that this man will therefore, according to the rules of mechanicks, travel 50 times more than the burden, and conſequently ſpend much time, they do not always allow this neceſſary augmentation of the way and of the time; nay, they ſometimes go ſo far, as to pretend that the rules of mechanicks are human inventions, which do not ſubject nature, and that we muſt believe experience rather than books.

This gave occaſion to M. *Amontons*, in a paper which he read to the academy, to review the firſt principles of mechanicks, and to lay open the whole compaſs of them, with regard to the uſe of machines. We will here give the metaphyſicks of them, which are no leſs demonſtrative than geometry, and may be more intelligible.

Whatſoever may be the cauſe of the reſiſtance of bodies to motion, at leaſt of thoſe upon which we can act, it is certain, that they bring a certain reſiſtance, and that it is the greater, as they are greater, or have a greater maſs. And as a quicker motion, is a greater motion, this reſiſtance is alſo proportioned to the velocity, with which one would move them. Conſequently, when a body is moved, the force which moves it is ſo much greater, as it has more maſs and velocity; ſo that the force, or *quantity of motion* of a body, is a product of its maſs by its velocity.

Of theſe two things, one is fixed, and always the ſame in all the motions which we can give to a body, and this is its maſs; the other, which is its velocity, is infinitely variable in the different motions which may be given it, and there is no body, how ſmall ſoever, that can go to ſuch a degree of quantity of motion, or force, as we will, provided we give it a ſufficient velocity.

Not only the ſame cauſe, which produces the reſiſtance of the bodies to the motion, produces a greater reſiſtance to a greater motion; but it does it only becauſe it produces the reſiſtance to the motion. Theſe two effects therefore are but one; therefore, the reſiſtance of a great body to a ſmall motion, is the ſame with that of a little body to a great motion, provided the maſſes and velocities are proportioned, and provided the greater quickneſs belongs to the leſs maſs. Therefore the forces neceſſary to ſurmount theſe reſiſtances are equal, and in like manner the quantities of motion of two bodies moved in theſe conditions.

Thence it follows, that the ſame force or quantity of motion in general, may be formed in infinitely different ways, and all equivalent; for provided the product of the maſs and of the velocity continues the ſame, theſe two magnitudes may vary infinitely between themſelves. But if we conſider a determinate body, to which we would give a certain quantity of motion, we cannot do it but by augmenting its velocity, ſeeing its maſs cannot be changed.

No force can be *in equilibrio* but with an equal force, nor be ſurmounted but by a ſuperior force. Thus all the art of mechanicks never renders a ſmall force equal or ſuperior to a greater; and every time that it ſeems, that a little force is *in equilibrio* with a greater, 25 *lb.* for example, with 100 *lb.* it is a ſort of illuſion upon the eyes, the *equilibrium* is not between 100 *lb.* and 25 *lb.* but between 100 *lb.* and 25 *lb.* ſo moved or diſpoſed, as neceſſarily to move four times faſter than the 100 *lb.* If we compare only theſe two fixed and immoveable magnitudes, 100 *lb.* and 25 *lb.* the 25 *lb.* ſeem to be multiplied and elevated above themſelves, and this is the falſe marvellous in mechanicks; but it is diſſipated by the 4 degrees of velocity, which were to be given to the 25 *lb.* and are a real and true force, tho' leſs ſenſible.

A force of 10 *lb.* moved 10 times faſter than the 100 *lb.* would have equalled them in like manner, and it is the ſame with all the poſſible products equal to 100; but at laſt we muſt always find 100 *lb.* force, in whatſoever manner we take them, either upon the fixed, or upon the variable magnitude; this is an inviolable law preſcribed by nature, which has left to art only the choice of the different combinations, which can make the ſame product of force.

A ſmall power, therefore, not being able to move a great weight, if it does not make as much way in the ſame time as the weight will make little, there happen from it two inſurmountable inconveniences; that the power, for inſtance, a man, is obliged to make a great motion, either of the arms or feet, for a long time, and that during this long time the weight, of which the elevation is the object of the whole machine, is but little elevated.

It follows from this, that the time neceſſary for the power to make a certain way regulates the time, during which the weight ſhall be elevated or moved. For example, if a man having a force of 25 *lb.* raiſes a weight of 100, this weight cannot be raiſed a foot in leſs time than the man could go 4, and conſequently it is from the man, or in general from the power that the time of the elevation of the weight takes the law.

Moſt engineers being dazzled, or willing to dazzle by an apparent multiplication of forces, either do not give attention enough to the time which it will neceſſarily coſt them, or do not know how to calculate it with ſufficient exactneſs; and this ſo eſſential a point not being cleared up, the machines are always wonderful as to the effect. Indeed, it is not eaſy to determine exactly what ſhall be the velocity either of men or horſes, which are the powers moſt commonly employed; and it is for this reaſon, that M. *Amontons*, after having carefully tried their velocities in different actions, has given the following reſults of his experiments, which will furniſh ſure and commodious principles for the calculation of moſt machines that can be imagined, and even of ſeveral labours that are undergone without machines.

Experiment	
1. Two chairmen loaded, going their uſual rate, made in 80″	70 toiſes.
2. A porter loaded, in 139″	
3. A footman walking, in 120″	
4. A footman running with his whole force, in 25″	
5. A drawer of a loaded calaſh, in 86″	
6. A horſe drawing upon the pavement a cart loaded with 1500 *lb.* in 112″	
7. Two horſes drawing a coach upon the pavement, at the uſual rate, in 62″	
8. Two other horſes drawing a coach alſo upon the pavement, in a trot, in 45″	
9. A ſaddle horſe, with its rider mounted, going the common pace, in 80″	
10. Another ſaddle horſe, with its rider alſo mounted, going the *grand pas* in 50″	

11. Some labourers carried each of them 22 baskets of earth in a day, each basket weighing 30 *lb.* to 370 toiſes diſtance, they came back empty, and this was certainly as much as they were able to do.

12. A man raiſed a weight of 25 *lb.* with a cord paſſing over a pully, to the height of 220 feet in 145″

13. A man who weighed 133 *lb.* roſe to the height of 10 toiſes 2 feet, on a ladder in 34″, and was quite out of breath, and unable to proceed.

14. Two horſes faſtened to a plough, in a ſoil neither too looſe nor too ſtiff, made each of them an effort of 150 *lb.*

15. A ſawyer gave 200 ſtrokes of a ſaw, and raiſed it as many times in 145″, at each ſtroke his hand went 18 inches, with an effort of 25 *lb.* he was very much out of breath, and could not go on above 3′ without taking breath.

We ſee now in what limits of time a great number of common actions are contained, and if we would make uſe of others, it will be eaſy alſo to make a fundamental experiment, which will regulate the whole calculation of the time.

When we ſhall know the time, which a certain power will neceſſarily require, to move a certain weight, we ſhall find perhaps, that the flowneſs will be ſo great, that it will be better to increaſe the power, and to renounce the deceitful or dangerous advantage of employing but a very ſmall one.

The whole art of mechanics in general conſiſts therefore, not in multiplying the forces, but, according to the expreſſion of M. *Amontons*, which ſeems more proper, in *modifying* them, and employing them againſt other equal forces, in the manner that beſt agrees with the deſign. The difficulty of chooſing well between the different manners is ſometimes great enough to exerciſe the beſt *genius's*.

In particular, there are an infinite number of attentions to be made to the moſt advantageous application of the moving force, or, which is the ſame thing, to its ſtrongeſt direction, to the means of hindering this direction from being changed in the actions which muſt be equal, or of making it, if it does change, to be recompenſed elſewhere, to the frictions, which are all put on the ſide of the weight againſt the power, which muſt conſequently be avoided and diminiſhed, as much as poſſible, to the ſituation of the centres of gravity or percuſſion, *&c.* Thus, when a machine anſwers its deſign well, there is no need to puff it up by a falſe multiplication of forces, to be ſure of its being one of the fineſt works of human genius.

XXII. *On frictions.*

The new diſcovery of M. *Amontons*, that frictions are always proportioned to the preſſure and to the velocity, and never to the ſurfaces, was important enough not to be received without a ſtrict examination.

There are ſome caſes, which ſeem evidently to prove the contrary. If, for example, two equal powers are applied to the extremities of a cord, which towards its middle is turned about a cylinder, and they draw one againſt the other; it is certain that one, in caſe it receives an augmentation, and becomes ſuperior, cannot make the other come to it, but by making the cord move upon the cylinder, and by ſurmounting its friction; and it is certain alſo, that it will have need of ſo much greater augmentation, as the cord ſhall make more turns about the cylinder. Now, why does a greater number of turns of the cord about the cylinder render the motion more difficult, unleſs it is becauſe the friction of the cord is made upon a greater quantity of parts of the cylinder, or upon a greater ſurface? for whatſoever number of turns the cord makes, the preſſure cauſed by the two powers is always the ſame.

M. *Amontons* anſwers to this, that it is always the preſſure that renders the motion more difficult, and that it is ſo much the greater, as the number of the turns of the cord is greater, becauſe we muſt reckon the ſame power as applied ſo many times to the cylinder, as the cord makes turns about it. Thus the power applied by a cord, which makes two turns, three turns, *&c.* becomes double and triple of itſelf. But as this multiplication of the ſame power might ſeem not very true and fit for the purpoſe, M. *Amontons* proves it much after the following manner.

Let the cylinder be cut according to its *axis*, into two equal parts; let one be ſuſpended and immoveable, and the other be moveable and placed lower, and let it bear a weight faſtened to it, and let a cord turn about both as often as you pleaſe, and let a power be applied to it, ſo that it may tend to make the lower half cylinder riſe, loaded with its weight, we ſhall ſee that the power is in the ſame caſe, that if it tended to make the weight riſe to the aid of a redoubled pully, and that conſequently, if there is an *equilibrium*, it is to the weight, according to the principles of ſtatics, as unity is to the number of half turns, which the cord makes about the cylinder; for unity is to this number, as the way, which the weight would make, to the way, which the power would make, in caſe the *equilibrium* ceaſed. All the effort therefore, all the action of the power conſiſts in this power multiplied by the number of half turns of the cord.

Now let us ſuppoſe the lower half cylinder raiſed by the action of the power, its plane ſurface applied againſt the ſurface of the upper half cylinder, and the weight taken away, which would have endeavoured to ſeparate them, all the reſt remaining the ſame, it plainly appears, that the power only preſſes one of theſe half cylinders againſt the other, that its action or effort are exactly the ſame as before, and that conſequently they always conſiſt in the product of this power, by the number of the half turns of the cord. Therefore independently on a greater ſurface of the two half cylinders being encompaſſed by the cord, and rubbed with it, the preſſure of the ſame power is greater by the multiplication of the turns of the cord.

The preſſure alone making by itſelf, according to M. *Amontons*, the whole reſiſtance of the friction, the greater or leſs velocity, which we give to the bodies that rub, is a circumſtance which augments or diminiſhes the effect of the preſſure, that is, the difficulty of the motion. We muſt therefore obſerve to diſpoſe machines in ſuch a manner, that the parts which rub may have the leaſt velocity poſſible. Thence M. *Amontons* concludes, that every time a wheel turns upon its *axis*, it muſt make the diameter of the *axis* the leaſt that is poſſible with regard to that of the wheel; for the two ſurfaces of the wheel and of the *axis*, which neceſſarily rub, will have the leſs velocity, ſince the velocity of a circular motion always diminiſhes from the circumference towards the centre. For the ſame reaſon, when we make indented wheels, we muſt make the teeth as ſmall, and as thin as poſſible. One of the teeth rubs by one of its ſurfaces, againſt an extent equal to the ſurface which rubs, and it muſt diſengage itſelf in a certain time, by running thro' a ſpace equal to the ſame ſurface. Therefore the ſmaller the ſurface is, the leſs ſpace it has to run through; and it muſt here be obſerved, that the ſmallneſs of the ſurface diminiſhes the reſiſtance of the friction, not becauſe it is a leſs ſurface that rubs, but becauſe it is a leſs ſpace run through.

Another obſervation of M. *Amontons* upon machines, with regard to friction, is, that we muſt be very careful that the line, according to which the moving force acts, may not be the ſame with that of the preſſure; and the reaſon of it is quite natural, for the moving force would unite its action to that of the preſſure, which is always contrary to it. This force muſt act as much as poſſible by the ſame line, according to which the bodies which rub are to move.

Notwithſtanding all theſe proofs and obſervations of M. *Amontons*, who had ſet his ſyſtem of frictions in a pretty good light, we are here obliged to acknowledge to the publick, that the academy was not fully perſuaded of it. They allowed that the preſſure was to be conſidered in frictions, and often to be ſolely conſidered, but they could not, with M. *Amontons*, abſolutely exclude the conſideration of the ſurfaces. He repreſented the great number of experiments, which he had made, where the ſurfaces went for nothing but experiments have a certain compaſs, more or leſs inſenſible, which may ſometimes be ſo on occaſions, where the determination of the queſtion might have been made. Thus they were willing to carry the affair, even into metaphyſics, and to ſeek what was to be thought of it in the firſt notions.

Let two bodies, having plain ſurfaces, be perfectly hard and poliſhed, and let them be moved one upon the other; it is plain that the reſiſtance cauſed by the friction, will either be none, or infinitely ſmall; but if, inſtead of this hypotheſis, which is not in nature, we conceive two bodies, having rugged and uneven ſurfaces, the difficulty of moving one of them upon the other, can come only from the neceſſity of raiſing the firſt to diſengage its parts let into the ſecond, or from the preſſing and rubbing of the parts of one againſt thoſe of the other, or of both together. In the firſt caſe, the only difficulty of raiſing one of the bodies makes that of the motion, and conſequently the friction comes only from the greatneſs of the weight to be raiſed, or, which is the ſame thing, from its preſſure, and the greatneſs of the ſurfaces goes for nothing. In the ſecond caſe, the greatneſs of the ſurfaces alone would do all, if it was poſſible that this ſecond caſe was abſolutely ſeparated from the firſt, that is, that the parts of one body were rubbed againſt thoſe of another, without raiſing either of them, for it is viſible, that a greater number of parts to be rubbed make a greater reſiſtance; but becauſe we do not rub without raiſing, at leaſt in practice, the reſiſtance, which comes from the greatneſs of the ſurfaces, is always mixed in this ſecond caſe with that which comes from the preſſure; whereas in the firſt, that which comes from the preſſure, may be ſingle and without mixture. Beſides that part of a body, which ſuffers a friction, is uſually very ſmall with regard to the great number of times, that it muſt be raiſed in the ſame friction, and to all the ſmall heights taken together, to which it muſt have been lifted. Thus, beſides that the reſiſtance, which comes from the preſſure, may be alone, beſides its always accompanying that which comes from the greatneſs of the ſurfaces, it is uſually much more conſiderable than that, when it does accompany it, wherefore in the greateſt part of the experiments, it is the only one that makes itſelf be perceived, and the only one that we ought to reckon. But as it is poſſible, that in certain caſes the preſſure may be very ſlight, and the number of parts to be rubbed very great, or what is to be rubbed may be very conſiderable, it muſt then happen, that the friction ſenſibly follows the proportion of the ſurfaces. This ſort of metaphyſicks, which we have juſt opened, may ſerve to give a more entire and perfect idea of the frictions, than that which might be drawn ſolely from experiments. When queſtions are of a nature to permit us to examine what ought to be, we may advance, that what ought to be well conceived, is alſo certainly that which is, and often ſets right what ſeems to be.

XXIII. *On the courſe held by ſeveral bodies, tied together by cords, and drawn on a horizontal plane.*

Suppoſe ſeveral weights, ſuch as you will, faſtened to the ſame cord, ſo that the parts of the cord contained between theſe weights being ſtretched as much as they can be, may make angles between themſelves, ſuch alſo as we will. The cord being drawn by any force whatſoever on a horizontal plane every where equally rough and rugged, if all the weights are moved together, it is certain, that they will have different courſes, from the directions, which they had before towards one another, which alſo were only the ſame parts of the cord where they are faſtened. Now the queſtion is to determine what all theſe different courſes ſhall be, and the force neceſſary to move all theſe weights. This is what M. *Parent* has done, by a general method, which contains all the particular caſes poſſible. We ſhall here give only the principles of them.

In the inſtant that the force, which draws a body on a rough horizontal plane, is ready to make it part, or which comes to the ſame, according to the geometry of infinites, in the inſtant that it makes it deſcribe a ſpace infinitely ſmall, it is exactly equal to the reſiſtance, which this body brings to the motion; that is, to its friction on a rough plane; for this is all that it has to ſurmount, in order to make it move. We ſhall here conſider only the reſiſtance cauſed by this friction, and the bodies drawn, only in the inſtant that they are ready to part, or deſcribe already ſpaces infinitely ſmall, and conſequently are *in equilibrio* with the moving force.

If two weights faſtened to the ſame cord are drawn by a force, the direction of which is in the ſame line with this cord, it is very plain, that provided this force is equal to the friction of the two weights, it will put them both in a ſtate of parting, and moving according to the line of its direction. But if the direction of the moving force is oblique to the cord, which binds the two weights, then it muſt be conſidered, that the *firſt* weight, that is, the moſt diſtant from the moving force, reſiſts the motion, which it endeavours to impreſs on the *ſecond*, which is the neareſt, and upon which we may conceive that it acts at firſt, that this ſecond being drawn by the force, according to its direction, it is therefore at the ſame time in a manner withdrawn a contrary way by the firſt; that the direction of the firſt with regard to the ſecond, is the very cord that binds them; that this cord being oblique to the direction of the moving force, the action by which the firſt weight reſiſts the motion of the ſecond, or draws it back, is therefore oblique to the direction of the moving force, and that conſequently according to the theory of compound motions, we muſt conceive this action of the firſt weight with regard to the moving force, as compoſed of 2 others, the one perpendicular to the direction of the force, the other parallel, or rather being in the ſame line with the direction of the prolonged force. This compound motion is the key of this whole matter. Seeing one of the two *ſimple* actions, in which the oblique action of the firſt weight with regard to the ſecond is reſolved or decompounded, is only the prolonged direction of the moving force, the firſt weight draws directly this way againſt the force, and conſequently the force cannot move the ſecond weight, without its being greater than the firſt. In this caſe, M. *Parent* retrenches from the direction of the force a part equal to the action of the firſt weight, or, which is the ſame thing, to its friction, and there remains a line, which repreſents the whole effort that the moving force can make according to its direction on the ſecond weight. But the firſt had alſo a ſimple action, or that which is perpendicular to the direction of the force. Now by this action, it is not at all oppoſed to the motion which the force tends to impreſs on the ſecond weight, for it might this way make the ſecond move, without diſplacing the firſt, that is, the ſecond would turn about the firſt, as about an immoveable centre, and conſequently there would not be any friction of the firſt weight to ſurmount. The line therefore, which repreſents this ſingle action of the firſt weight, remains entire; and laſtly the diagonal of a parallelogram formed of this line, and of that which remains of the direction of the force, will repreſent the whole action of the force on the ſecond weight, modified by the reſiſtance of the firſt, or, which is the ſame thing, the courſe which the ſecond weight muſt follow. As for the firſt, as it does not draw any other weight after it, and as its motion is not modified by the reſiſtance of any other, the direction of its motion, or the courſe which it takes, is the ſame line with the cord which binds it with the ſecond weight.

The moving force therefore makes the two weights move according to two, different directions; the firſt, according to the cord which binds it to the ſecond; the ſecond, according to the diagonal which we have explained, and conſequently this force may be repreſented by theſe two different lines, and conceived as equal to their ſum, with regard to the other actions or reſiſtances repreſented by other lines.

The force and the two weights continuing the ſame, if the direction of the force is ſuppoſed more oblique to the cord of the two weights, the ſingle action or reſiſtance by which the firſt weight draws the ſecond back directly againſt the force, becomes the greater, and it may become ſo to ſuch a point, that the force will be no longer capable of moving the ſecond weight this way, but only of making it turn about the firſt, which will be immoveable. The *equilibrium* therefore here, as uſual, depends on the greatneſs and direction of the weights or forces.

If the force was not great enough to overcome the friction of the ſecond weight, it could never move the firſt, which cannot abſolutely proceed without the ſecond.

Now, if inſtead of two weights, we ſuppoſe three bound by cords, which make angles between themſelves, we muſt make upon the the third and ſecond the ſame reaſonings, which M. *Parent* has made upon the ſecond and firſt. The ſecond will draw back the third in a contrary way to that of which the force will draw it; and becauſe the direction of the ſecond is ſuppoſed oblique to that by which the force draws the third, this third will deſcribe a diagonal, which will be found as that of the ſecond was, when there were but two. But it muſt be obſerved, that in this caſe, where there are three, the courſe or diagonal of the ſecond is no longer the ſame that it was, and the reaſon of it is, that the force draws it no longer according to the ſame direction as before, ſince we ſuppoſe it draws it by an oblique direction to the cord which binds the ſecond and third. Now it is viſible, that the direction, by which the ſecond weight is drawn, cannot change without its courſe changing. This alteration of the courſe of the ſecond weight, when there are three, has eſcaped a geometrician of the firſt rank, who has examined the ſame caſe, and M. *Parent* thinks himſelf the firſt, who perceived this ſlight inadvertency.

The caſe of the three weights being well underſtood, a greater number of weights does but render the application of the principles a little more complicated and difficult. The courſe of the bodies being found, the value of the force which makes them deſcribe theſe courſes, is ſoon found; it is nothing but a calculation, which we leave to the algebraiſts, and it is ſufficient for us to have given the ſoul of it.

AN
ABRIDGMENT
OF THE
Philoſophical Memoirs
OF THE
ROYAL ACADEMY of SCIENCES at *Paris*, for the year 1703.

I. *Obſervations on the quantity of rain that fell at* Paris *at the royal obſervatory, on the thermometer, and on the barometer, during the year* 1702, *by M.* de la Hire *.

THE year 1702 has been looked upon as one of the moſt extraordinary that have been for a long time, for the drought of the ſpring and ſummer. Yet it has been one of the moſt fruitful in grain, excepting the ſummer corn, which may be aſcribed to the natural coolneſs and moiſture of the lands of this country. It is alſo obſerved, that when the end of ſpring and beginning of ſummer are rainy, the weeds grow very much, and turn up the corn; and beſides in moiſt ſeaſons there often come fogs, which ſpoil the grain when it is beginning to form.

The quantity of water which fell in rain, or in ſnow, was in the months of

	Lines.		*Lines*
Jan.	18 ¼	July	19
Feb.	18	Aug.	35 ¼
Mar.	9 ½	Sept.	11 ¼
Apr.	17 ½	Octob.	15 ½
May	5 ¼	Nov.	18
June	9	Dec.	18

---

* Jan. 10, 1703.

And ſo during the whole year the ſum was 196 lines, or 16 inches $\frac{1}{2}$, which is much leſs than in the common years, which give 19 inches of water. But it muſt be obſerved, that very often the 3 months of *June*, *July*, and *Aug.* furniſh as much as all the reſt of the year, which did not happen this year, ſince theſe months were not different from the reſt. And tho' the water, which falls in theſe 3 months, is in great abundance, which comes only from ſome ſtorms that go over quickly, it contributes but little to the nouriſhment of plants, as it is ſuddenly raiſed in vapours by the great heat of the earth and air, or runs off into the ponds and brooks, without penetrating far into the earth.

My thermometer is always ſituated in an uncovered place, where neither the ſun nor wind can affect it, and is filled with ſpirit of wine coloured, and is ſealed hermetically.

It was at the loweſt this year at 14 degrees $\frac{1}{2}$ *Jan.* 1. which was a ſign of very great cold; but from the 6th it roſe to 43 degrees, and it kept up all the reſt of that month, and even at the beginning of *February* to about 40 degrees, ſometimes a little more, and ſometimes a little leſs; tho' the cold is uſually greater at the beginning of *Feb.* On *Dec.* 25. it was at 44 degrees $\frac{1}{4}$, which is within about 2 degrees as high as it was in the beginning of *June.* On *Dec.* 17. it was the loweſt ſince the beginning of winter at $30\frac{1}{3}$.

June 19, it was at 60 degrees $\frac{1}{2}$, *July* 29, at 61, and *Aug.* 6, at 62, which is the higheſt it came to; but *Sept.* 2. it was again at $61\frac{2}{3}$. Whence we know in general, that during the months of *June*, *July*, *Aug.* and the beginning of *Sept.* there were very great heats. The greateſt height of the liquor of the thermometer was about 2 in the afternoon at 71 degrees ½. *Aug.* 5. There fell no ſnow, or but very little, either at the beginning or end of this year.

The quickſilver roſe in the tube of my barometer to 28 inches, 2 lines ⅓, *Feb.* 11. The wind was then S. as it was for ſome days before and after, tho' the barometer always kept to about 28 inches, which does not agree with what we uſually obſerve of the height of the quickſilver in the barometer and of the wind. It was at the loweſt 26 inches, 5 lines *Dec.* 20. with a moderate S. wind, which is very extraordinary; for this great falling of the quickſilver ſeemed to indicate ſome great ſtorm. The difference therefore between the higheſt and the loweſt, was 1 inch, 9 lines ⅓, much more than uſual.

M. *Amontons* having propoſed this year to the academy a thermometer, like that of *Sanctorius*, which is not ſealed, and having the degrees of heat marked by the elevation of the quickſilver in a tube, inſtead of *aqua fortis* that had been uſed in engraving, which *Sanctorius* employed, the heat and cold of the exterior air acting on that of the phial of the thermometer, and having obſerved that the boiling water, in which he had plunged this phial, could cauſe but a certain dilatation of the air incloſed therein, let the fire be ever ſo much augmented to make the water boil, he judged to be a mean of having a term known thro' all the world, to which the different degrees of dilatation or heat of the air might be referred; and having given me one of theſe thermometers, filled after the manner explained by him to the academy, I expoſed it to the ſtrong ſummer ſun behind a glaſs, about noon, in a place cloſe ſhut up, and I obſerved that the quickſilver roſe in it at the higheſt to 31 inches, 5 lines, about 2 in the afternoon, *Aug.* 5. The quickſilver of my barometer was then at 27 inches, 10 lines $\frac{1}{3}$.

The declination of the needle was *Sept.* 22, 1702, 8° 48′ towards the W. It was obſerved in the ſame place, as in the preceding years, and with the ſame needle, 8 inches long.

II. *The thermometer reduced to a fixed and certain meaſure, and the means of referring to it the obſervations made with the old thermometer, by M.* Amontons *; *tranſlated by Mr.* Chambers.

We are all agreed, that heat is the general cauſe of all the effects, and all the productions upon our earth; and that without this, the whole would be a fixed motionleſs maſs. Heat, therefore, being as it were the ſoul of nature, 'tis of the utmoſt importance to philoſophers, to be able to meaſure it with exactneſs; ſo that the inſtruments which ſerve to determine the ſeveral degrees thereof, cannot fail of being cultivated in a ſcience, whoſe whole object is the knowledge of ſuch effects and productions.

But as this knowledge is not the work of a day, but rather of many ages; and as there is no arriving at it, but by a long ſeries of obſervations, the application whereof frequently cannot be found till long after they have been made; one of our chief cares ſhould be to preſerve and tranſmit them to poſterity, who may reap the fruit of their fathers pains; thus, for inſtance, it is, that by the length of the pendulum, which vibrates ſeconds, we have already aſcertain'd all ſuch as depend upon meaſure; and thus by the

---

* April 18, 1703.

equilibrium, which we have found means to make between the atmoſphere and other fluids, whoſe weight is known, we may perhaps be able to aſcertain all ſuch as depend on the air wherein we live, which, according to many, contains the firſt principal of life, and which therefore cannot be too much cultivated.

Sanctorius, in his commentary on *Avicenna*, has left us ſeveral methods, whereby he apprehends this might be attain'd; but the thermometer he gives us, acting at leaſt as much by the weight or lightneſs of the air, as by its heat or cold, the common ſpirit of wine thermometers are juſtly preferr'd thereto; and for the like reaſon, the thermometer lately invented by me, is preferable to thoſe others; I may add, that being free from all the defects of the former thermometers, there would be nothing further to be deſir'd on this head, were it but eaſily portable, and no neceſſity in uſing it to rectify it by the weight of the air.

But as the utmoſt care is to be taken, not to invert it, which would intirely diſcompoſe it; and as in every time of obſervation, regard muſt be had to the greater or leſs weight of the atmoſphere, in order to allow for what it exceeds or comes ſhort of 28 inches; it will not ſuit all kinds of perſons. Upon the whole, its chief uſe is, for perpetuating to future ages the obſervations made by our thermometers, and to ſerve as a ſtandard, whereby to regulate all thermometers which have the ſame ſcale. The manner of effecting which, we proceed now to ſhew.

To regulate a ſpirit of wine thermometer by an air thermometer.

* As to the figure of the glaſs, and the liquor contained in it, the new ſpirit thermometer does not differ from the common ones, excepting its magnitude and ſcale, which is exactly equal to the ſcale of an air thermometer, when rectified by the weight of the air; its graduation is by inches and lines, of which there are two orders, one aſcending, the other deſcending.

The aſcending ſcale ſhews the degree of heat, that is, the quantity of inches and lines height of mercury, which the heat enables the ſpring of the air to ſuſtain; from whence it follows, that the extreme degree of cold in this thermometer, would be that which ſhould reduce the air, not to ſuſtain any weight of mercury by its ſpring, which muſt be a degree of cold much more intenſe, than what we commonly call extreme cold, ſince by experiment it appears, that if the heat of boiling water enables the ſpring of the air to ſuſtain a weight equal to that of 73 inches of mercury, the degree of heat which remains in the air, when water freezes, is ſufficient to enable it to ſuſtain 51 inches $\frac{1}{2}$, which deſerves particular notice.

The graduation which deſcends, ſhews the degrees of cold below the heat of boiling water, that is, the number of inches and lines, whereby the diminution of heat, below that of boiling water, enables the air to ſuſtain leſs mercury.

Now theſe degrees of heat and cold, indicated at the ſame time, by the new thermometer, are always the complements of each other to 73, which ſhews, that when one is known, the other is likewiſe known, the whole as repreſented by the adjoining

---

* Plate I. Fig. 2.

ing figures, which ſhew wherein the graduation of the new thermometer differs from that of the old one.

By this new thermometer we have found, that the greateſt and leaſt degree of heat, experienced at *Paris*, are nearly to each other, as 6 to 5; ſo that between the greateſt heat of ſummer, and the ſevereſt cold of winter, the difference is only about a 6th part. Hence, as in our winter's cold, a great part of the fluid bodies loſe their fluidity, 'tis highly probable, that in an abſolute cold, wherein there were no remains of heat, no fluid body would be excepted from congelation; which ſeems to prove, that ſolidity is the natural ſtate of all bodies.

Tho' the thermometers which now obtain, will at beſt only ſerve to make a groſs compariſon of the degrees of heat, without meaſuring them, yet, as multitudes of obſervations have been made therewith, that poſterity may not be altogether deprived of the benefit of them, we ſhall ſubjoin a method of rectifying them, by comparing the thermometers uſed with that here deſcribed.

To rectify obſervations made with the old thermometers by the new one.

Place a new thermometer for ſome days, aſide that which has been uſed in making obſervations, and after an hour or two, note at what degrees both of them ſtand; in a few days you will find them conſiderably alter'd; note their degrees as before, then divide the whole ſpace, which the old thermometer has moved, into as many parts, as the ſpace moved by the new one contains lines, and by theſe parts make a new graduation aſide of the old one, diſtinguiſhing them by 12 and 12, in the ſame order, and mark'd by the ſame figures as in the new thermometers, with which they will perfectly agree, excepting that they will be greater or leſs, as thoſe thermometers are more or leſs ſenſible.

To diſtinguiſh theſe diviſions from the reſt, they may be called reduced lines, 12 whereof will make the reduced inch, ſo that the two thermometers will at the ſame time point out the ſame inches and lines, that is, the ſame degrees of heat and cold; this done, it will be eaſy to reduce the degrees of any former obſervations, into degrees of heat or cold, whoſe effect is better known.

Suppoſe, for inſtance, it were required to know what the degree of heat was, when the old thermometers ſtood at the 50th diviſion of their ſcale? after making a new ſcale, as already directed, we ſhall find that the place, correſponding to ſuch 50th diviſion, will be at 54 inches on the aſcending, and 19 on the deſcending ſcale, which ſhews, that the degree of heat, which raiſed the old thermometer to the 50th diviſion, is the ſame which enables the air, in the ball of the new thermometer, to ſuſtain 54 inches of mercury, and that this degree is about $\frac{1}{4}$ of that of boiling water.

III. *Remarks on rain water, and on the origin of ſprings; with ſome particularities concerning the conſtruction of ciſterns, by M.* de la Hire *.

All that concerns water, both for the neceſſities of life, and the ornament of palaces and gardens, has always been regarded as one of the principal ſorts of knowledge, neceſſary for men. The greateſt care has been taken to render the very ſmalleſt rivers capable of carrying great boats, and by this means to join very diſtant ſeas together, Very plentiful ſprings have been brought by long turnings, and upon very elevated aqueducts, even to places where nature had refuſed to ſupply any. In ſhort, a great number of machines have been invented, to raiſe water, and to carry it even to the top of mountains, to diſtribute it afterwards under a thouſand different figures, with ſupernatural motions, and to make a ſight of it, worthy of admiration. This was enough for people in general, but the curioſity of thoſe, who inquire into the ſecrets of nature, was not enough ſatisfied; they wanted to know the origin of theſe ſo abundant ſources of water, which are found all over the earth, and even upon very high rocks, and this is what has ſo much exerciſed the ancient and modern philoſophers.

---

* April 18, 1703.

We find two principal opinions concerning the origin of ſprings, each of which is founded upon experiments, which ſeem indubitable; for it is evident, that ſeveral ſprings have for their principle the rain water, and ſnow melted upon the mountains; but how can theſe rains and theſe ſnows, which are very rare upon ſteep and high rocks, and in very hot countries, furniſh the very plentiful and laſting ſprings, which we ſee in ſeveral places.

The reſt of this paper is tranſlated by Mr. Chambers.

But the great objection is, that we have ſprings even upon high and ſteep rocks, and in the hotteſt countries, where little rain or ſnow is known to fall. Others, therefore, chuſe to have recourſe to certain ſubterraneous cavities in form of alembicks, wherein the vapours, iſſuing from the ſubjacent ſea-water, and riſing thro' the clefts and crannies of rocks, are condens'd by the cold of the ſurface of the earth.

M. *Mariotte*, to ſupport the former, made an exact eſtimate of the quantity of rain and ſnow, which falls on the tract of land that furniſhes the water of the river *Seine*, and found, that much more fell than was neceſſary to maintain the river at a moderate height, during the courſe of a year.

Upon reading the treatiſe of Dr. *Plott*, *De Origine fontium*, printed 1685, I was led to make a like examination myſelf of the quantity of water, which the rain and ſnow may furniſh towards the formation and maintenance of ſprings and rivers; and I began with enquiring, what quantity of rain-water falls on the ground in the compaſs of a year; the reſult whereof I have yearly communicated in the memoirs of the academy, by which it appears that the depth of water, which falls at the royal obſervatory, *communibus annis*, amounts to between 19 and 20 inches, which agrees pretty nearly with what M. *Mariotte* had found.

But being in doubt, whether this whole quantity of water might be ſuppoſed to go to the formation of ſprings, I made the following experiments to ſatisfy myſelf hereof——chuſing a place in the lower terras of the obſervatory, I planted a leaden baſon therein eight feet under ground, the baſon was four feet in ſurface, and ſix inches deep, being a little inclin'd towards one of its angles, to which I had ſolder'd a leaden pipe 12 feet long, one end whereof, after a conſiderable deſcent, came into a cellar; the baſon was contrived at a diſtance from the cellar-wall, that it might be ſurrounded with a greater quantity of earth, and might not be kept dry by the proximity of the wall; cloſe by the orifice of the pipe in the baſon I put ſeveral flints of different ſizes, to prevent the orifice from being ſtopp'd, when the earth ſhould be thrown over it to the depth of eight feet. The ſoil is of a middle nature, between ſand and loam, ſo that the water might eaſily penetrate it, and its upper ſurface is quite level.

Now I apprehended, that if the rain and ſnow water penetrate the earth, till they meet a clay or compact earth to ſtop them, the baſon muſt here have the ſame effect, ſo that I might expect to have a ſpring of water burſting into the cellar.

But having no great opinion that this would ſucceed, I placed another baſon at the ſame time, only eight inches under ground, the ſurface of which laſt was 64 inches, and its ſide or rim eight inches high; for this purpoſe I had choſe a place ſcreen'd both from wind and ſun, and took care to pull up all the graſs and other herbs which grew over the baſon, that all the water, which ſhould fall on the ground, might paſs uninterrupted to the bottom of the baſon, wherein was a little hole, with a tube to convey the water into another veſſel.

The effect was, that in all the ſpace from the 12th of *June* to the 19th of *Feb.* following, no water came by the tube from the baſon; and tho' it began to run now, this was intirely owing to the great quantity of ſnow which had fallen, and was now melting. From that time the earth in the baſon was always very moiſt, tho' the water would only run a few hours after raining, and it ceaſed running, when the quantity fallen was drained off; for ſome always remained in the earth, and would not go off, unleſs ſome new rain chanced to fall.

A year after, I repeated the ſame experiment, but only buried the baſon 16 inches under ground, which was double its former depth. I took care likewiſe, that there were no graſs on the ground, and that it might be ſcreened from the ſun and wind, which would dry it too faſt. The effect was much the ſame as in the former, excepting only that when a conſiderable time paſſed without raining, the earth would grow a little dry; ſo that a moderate rain coming on, it would not moiſten it ſufficiently to make it run.

Laſtly, I planted herbs on the ground over the baſon, but found, that when theſe were grown up a little, the ground was ſo far from ſending me any water after rain, that all that fell was not ſufficient to ſuſtain them, but they would droop and wither, unleſs ſprinkled from time to time with water.

Upon this, I bethought myſelf to meaſure the quantity evaporated thro' the leaves of plants, when expoſed to the ſun and wind, and accordingly on the 30th of *June*, at ½ an hour paſt 5 in the morning, I put a pound of water, weighed very exactly, in a glaſs phial, with a narrow neck, and gathering 2 leaves of a fig-tree, of a moderate ſize, which together weighed 5 drachms, 48 grains, I immerged the ends of their foot ſtalks in the phial of water. Theſe leaves when gathered, were very freſh and firm; but after expoſing them with the phial to the ſun, which was very hot, and in a place where there was but little wind, and ſtopping the neck of the phial left vacant by the foot-ſtalks very exactly with paper, that no water might evaporate this way, I weighed the whole again at 11 the ſame morning, and found, that there had been a conſumption of 2 drachms, which the ſun and air had drawn away from the moiſture of the leaves, and which can only be repaired, when the leaf is growing to its tree, by the moiſture of the earth convey'd to it thro' the roots.

I alſo made ſeveral other experiments upon plants, and always found a great diminution of their moiſture; in effect, having meaſured the ſurface of ſome leaves, and conſidered the quantity of them, which uſually covered the ground, I apprehended, that the rain was by no means ſufficient; eſpecially in ſpring, to ſupport them, without ſome foreign aſſiſtance. 'Tis true, the air and dew of the night ſupply the larger trees, and even plants, with a great ſtock of moiſture, which we commonly find on the leaves about the ſun-riſe, and which deſcended into the roots, may help to ſupport them part of the day; but this moiſture alone would by no means ſuffice, were they not to borrow a great quantity from the earth, and the rain which pierces the ſame, as may appear from the following experiments.

What has been ſaid abundantly ſhews, that the rain which falls on the ground where there is graſs, or other plants, cannot penetrate it 2 feet deep, excepting, perhaps, in gravelly or ſtony places, where it may ſink with more eaſe. Of this, we have an inſtance in the rock of St. *Baulm* in *Provence*, where the rain falling on this rock, which is all cleft and chinky, as well as void of graſs and other plants, penetrates in a few hours time into a grotto, 67 fathom below the ſurface of the rock, and forms a beautiful ciſtern. Hence on ſuch rocks, and in large ſolutions of ſnow in ſummer, we frequently find certain ſprings overflow at a great rate, during certain hours of the day, and this even ſeveral times in a day, in caſe the ſun only ſhine on the ſnow in certain different parts of the day, the points of the rocks, *&c.* keeping it in the ſhade all the reſt. This we take to be the reaſon of thoſe ſprings in the middle of the land, which have an ebbing and flowing like the ſea.

Theſe experiments convinced me, that I was not to expect the rain or ſnow water to paſs thro' 8 feet of ground into the firſt baſon, which I had lodged for that purpoſe; accordingly in the 15 years that it continued there, it never yielded a ſingle drop.

Whence it appears, that 'tis but few ſprings can owe their origin to rain and ſnow; ſo that we muſt of neceſſity have recourſe to other cauſes, in order to account for the formation of ſuch plenteous and perenneal ſprings, as we frequently find in high places, and at a very ſmall depth under ground. For inſtance, that of *Rungis* near *Paris*, which, however, is equally inexplicable, from the ſuppoſition of thoſe ſubterraneous alembicks, or grottos, which ſerve to collect and condenſe the vapours of the ſea water; for there are no rocks in the country all round, as I have learnt by ſeveral wells, which I have ordered to be dug, and that the ground is but little raiſed, and the water very near the ſurface. This ſpring ſupplies 50 inches of water in a continual ſtream, without any ſenſible change or interruption, and yet the whole ſpace of ground which it can ariſe from, would not afford ſo much water, were all the rain that falls on it collected and none ſpent any other way; whereas the ground is all tilled, and covered either with graſs or corn; add, that in ſome vallies near this place, they are forced to dig very deep ere they come at water. Others have endeavoured to account for theſe ſprings, by ſuppoſing certain natural canals to convey water from ſome high river, and which paſſing over high and low places, and even perhaps under rivers which come a-croſs them, are ſo cloſe and well lined, as not to let their water eſcape by the way, but bring it into the place where it burſts out of the ground: but admitting the reality of ſuch ſubterraneous canals, I am ſatisfied, they could only have a deſcent neceſſary to make the water flow upon a clay bottom; but to ſuppoſe natural tubes high and low ſtill correſponding, is as much as art can compaſs in a little garden.

Yet one further objection lies againſt this hypotheſis; for if theſe high ſprings ariſe from rivers, thoſe rivers muſt derive their water from other ſprings ſtill higher; for thoſe formed by rains and melted ſnow in places where the bottom is firm, can only form brooks or torrents, which will be of ſhort continuance, and by no means able to ſupply the continual flux of ſuch rivers. The large collections of water, as ponds frequently found at the head of ſmall rivers, prove nothing as to the origin of rivers; ſince, by ſeveral experiments we have found, that more water exhales from a wide veſſel, than can fall into it by rain.

There only remains one method of accounting for the origin of theſe ſprings; and this too is liable to ſome difficulties. ——— We are to ſuppoſe then a great quantity of vapours, to paſs thro' the earth, being raiſed from funds of water uſually found therein, about the level of the ſea, or the neareſt rivers; that theſe waters paſs the more freely, as they meet with a ſoil eaſier to be penetrated; ſomething of which we find upon opening deep pits in the winter. Now the particles of ſuch vapours may be united again and condenſed into water, either by the coldneſs of the ſurface of the earth, as they approach thereto, or by meeting with a ſoil already ſoaked with water, or by ſome other matter which may be diſpoſed to fix them, as we find that ſalts expoſed in the air catch, and retain the watery particles floating therein. Upon this the water growing ſtill in quantity, and coming at length to meet with a bottom firm enough to ſuſtain it, glides along the ſame, till at length it either breaks out on the ſurface of the earth, where its firm bottom happens to terminate, or ſinks lower into the ground, in caſe it meets with apertures in the clay bottom. —— This ſeems to me the moſt probable account; and yet in this we are to conceive, that the vapours have particular canals whereby they paſs, and which are ſo cloſe, that the water formed of them cannot eſcape through the ſame.

Being deſirous to know by experiment, what might be expected from the manner of condenſing the vapours by their faſtening upon ſtones, repleniſhed with ſalts, I put a glaſs veſſel in one of the cellars, at the bottom of the quarry of the obſervatory, and on the edge of the veſſel faſtened a piece of linnen, which I had dipped in water, wherein ſalt of tartar had been diſſolved. This ſalt I choſe, as apprehending it more diſpoſed to fix the vapours than any other; add that the place appeared very moiſt, eſpecially in ſummer. Some time after, I found the bottom of the veſſel filled with a liquor, which was only the water produced by the vapours in the air, which gathering upon the linnen, till it was ſaturated therewith, the ſurplus had trickled down along the ſides of the veſſel. This experiment I would have carried further, to ſee whether the liquor would have continued running, and whether the ſalt in the linnen were carried off by the water; tho' 'tis poſſible, that ſtones, containing ſalts fit to fix the vapours, might ſtill preſerve their ſalt, and even acquire new: but, in my abſence, ſome people coming into the cellar, broke the veſſel, and put an end to the experiment.

I ſhall not here inſiſt on ſome extraordinary kinds of ſprings, found by the brink of the ſea, and upon high rocks, which ebb and flow like the ſea itſelf, and yet afford freſh water. This I have elſewhere accounted for mechanically, by ſuppoſing ſubterraneous reſervoirs, a little above the level of the ſea, and that the cavity wherein they are placed, communicates with the ſea by certain canals.——For it muſt follow, that when the ſea riſes, it will compreſs the air in theſe cavities, which, preſſing upon the water in the reſervoir, will oblige it to iſſue, and even ariſe thro' certain clefts or paſſages of the rocks, till it arrive at the ſurface of the earth, where it will form a ſpring, which muſt gradually diminiſh, as the ſea ebbs or retires; that is, as the air, before compreſſed, returns to its natural ſtate.

Thus much for the origin of fountains: I ſhall now proceed to ſome obſervations, which I have made on this occaſion, as to the advantages which may be drawn from rain water.— The moſt conſiderable is, that of collecting it in ſubterraneous reſervoirs, called ciſterns; where, after it has been purified by paſſing thro' river ſand, it will keep many years without ſpoiling. This water is uſually the beſt that can be uſed, either for

drinking,

drinking, or for the other œconomical purpoſes, as waſhing, bleaching, dying, *&c.* by reaſon it is not mixed with any ſalt of the earth, as all ſpring waters generally are, ſuch ciſterns will be of great advantage in places where there is no ſpring water, or even where all the well waters are naught. I need not here enter upon the conſtruction of ſuch ciſterns, nor the choice of the materials to be uſed therein, ſince all that is required is to have a place which will hold water, and that the ſtones and the mortar they are joined with, may not communicate any vitious quality to the water, which is to continue there a conſiderable time.

Thoſe who have ſuch ciſterns, and are curious of their water, take care not to let any water of melted ſnow, or even of ſtormy rains, enter the ciſtern. The former, I think, they have ſome reaſon to exclude, not on account of any ſalts, which they imagine mixed with the particles of ſnow, but only becauſe ſuch ſnows uſually lie ſeveral days, and ſometimes whole months on the roofs of houſes, where they are ſpoiled and corrupted by the dung of birds and other animals, as well as by their long continuance upon tiles and ſlates, which are always very dirty; for this reaſon, when it begins to rain, I would have the firſt water which falls from the roof, be thrown away as naught, having only ſerved to waſh the roof, which is always covered with duſt, raiſed from the mire dried in the ſtreets and roads, and only admit that which comes afterwards into the ciſtern.

The following obſervation relating to the water which is to be excluded from ciſterns, I fell upon accidentally, having been engaged for ſome years in collecting the water which falls at the obſervatory,

ſervatory, by means of an iron baſon tinn'd over as already deſcribed in another paper; I uſed to waſh the baſon as well as the veſſel, into which it conveys its water, as quickly as poſſible, at the beginning of any rain, which promiſed to be conſiderable, and put up the water as it came into clean glaſs bottles, to preſerve it; but being deſirous at length to taſte this water, I was ſurpriſed to find it very ill ſavoured, and that it had a caſt of ſmoke, which to me appeared very extraordinary, as I had often taſted other water gather'd in the ſame way, which had nothing of this taſte, nor could I ſee any thing that might give it this ſmoaky taſte; the place where it was collected being very open and high, and no chimney near it; but conſidering at length that this water fell with a north wind, which is not very common, that being a wind when it rarely rains, and that the whole city lies to the north of the obſervatory, the ſmoke of the chimnies might eaſily have mixed with the rain, and thus become collected along with it, which would naturally give it the taſte abovementioned; but of this I was further aſſured ſometime after, for gathering ſome rain-water which fell with a ſouth or ſouth-weſt wind, on which quarter there is nothing but open fields, I found no ſigns in it either of the ſmoaky taſte or ſmell, from whence it follows, that all thoſe rains are alſo to be kept out of the ciſtern, which are brought by winds which paſs over places infected with any ill ſmell, as ſinks, lay-ſtalls, large cities, *&c.* for that a little water, impregnated with any noiſome vapours and exhalations, coming to mix in the ciſtern, will corrupt all the reſt.

In effect, it being evident from all experiments, that rain water purified in river ſand, of all its mud and earthy ſmell, is both the moſt wholeſome and uſeful of all others; I have thought of the moſt convenient method of preparing ciſterns, to furniſh every houſe with a ſufficient quantity of water——in order to this, it is firſt obſervable, that a common houſe about 40 fathoms in ſurface, if properly roofed over, will gather every year about 2160 cubic feet of water, reckoning only 18 inches *per annum*, which is the leaſt I have known fall. Now theſe 2160 cubic feet, will be equivalent to 75600 pints of water, at the rate of 35 pints to a foot, which is the juſt proportion for the *Paris* pint; dividing therefore this number of pints by the 365 days of the year, we ſhall have 200 pints a day; ſo that ſuppoſing 25 perſons to live in the houſe abovementioned, there will be eight pints of water *per* day for each, which is more than ſufficient for all the uſes of life.

It remains only to give ſome inſtructions, relating to the place and manner of building ſuch ciſterns in private houſes: in ſeveral towns of *Flanders*, near the ſea-coaſt, where the well-waters are all brackiſh and bitter, by reaſon the ſoil is only a light ſand, which the ſea-water can paſs thro' without being purified and ſweetened; every houſe has its ſeveral ciſterns, but theſe ciſterns are under ground, and are only a ſort of cellars, where they imagine water will keep better than in the open air; and in effect, thus much is true, that water, eſpecially that of rain, will not keep in the air, by reaſon of the mud it abounds with, which it has not wholly depoſited and left behind it in paſſing thro' the ſand, ſo that it will corrupt, and a greeniſh kind of moſs grow over its ſurface; for this reaſon I would recommend a place to be contrived in every houſe, about ſix feet above the level of the reſt, eight or 10 feet high, and only a 40th or 50th part of the ſurface of the earth, which in our inſtance would amount to about a fathom. This place being built round with thick walls, a leaden ciſtern ſhould be lodged in it, into which all the rain water ſhould be convey'd after paſſing thro' a proper quantity of ſand; a very ſmall door would ſuffice for it, which being very thick withal, and matted over, would hinder the froſt from reaching the water; and from ſuch a conduit might the water be eaſily diſtributed into the kitchens, *&c.* This water being well ſcreened, would keep as well as if under ground; and its being a little elevated above the level of the ground, would ſerve for its commodious diſtribution to all the lower parts of the houſe.

I have lately examin'd the ſeveral rain waters which I had formerly collected, and kept in glaſs bottles, and find, that tho' ſome of them have a very ill taſte, which might be owing originally to the ſmoke, or other circumſtances above mentioned; yet others remained perfectly good and pleaſant, without any thing of that earthy taſte found in all rain waters. I ſuppoſe this is owing to their having ſettled and depoſited their mud, which we commonly find at the bottom of veſſels, where rain water has ſtood any time.

I ſhall cloſe with one further remark, which I made on ſome ſpring water, near *Montmarte*; this is a clear and good water for drinking, yet if fleſh and the common potherbs be boiled with it, the broth will be extremely bitter, which cannot be owing to the nature of the herbs of the place, ſince if rain water be made uſe of, the broth will not have the leaſt caſt of bitterneſs.

IV.

IV. *That the new experiments on the weight and elaſticity of the air ſhew, that a moderate degree of heat, may reduce the air into a ſufficiently violent ſtate, to be the ſole cauſe of very great earthquakes and commotions on the terreſtrial globe, by M.* Amontons *; *tranſlated by Mr.* Chambers.

This ſurpriſing paradox is wholly founded on this, that we know no bounds of the condenſation of the air, any more than of its dilatation; and that this peculiar property it has of being reducible by preſſure, into bulks reciprocally proportional to the weights it is compreſſed withal, may make it ſurpaſs many times the weight even of the heavieſt bodies, and increaſe the force of its ſpring accordingly, in which ſtate it acts upon it with great violence; for tho' it has been affirm'd by Dr. *Halley*, in the philoſophical tranſactions, that according to the experiments made in *London*, and in the academy *del Cimento*, no power is capable of reducing air into a bulk 800 times leſs than what it poſſeſſes on the ſurface of the earth. As he does not relate thoſe experiments, which muſt needs be very difficult to make with exactneſs, and wherein of conſequence it will be eaſy to be deceiv'd; and as on the contrary the experiments I have made, convince me, that the ſpring of the air, conſiſting only in the motion of the fiery particles among which it floats, and wherewith it is continually pervaded, it does not appear that we can expel them entirely by any force whatever, nor conſequently reduce the air to an incapacity of further condenſation ; for 'tis evident, while any matter remains between its particles, ſo fluid and moveable as we conceive the corpuſcles of fire to be, nothing can hinder this condenſation of the air from continually increaſing more and more, provided the cauſe which produces it be increaſed accordingly.

---

* May 19, 1703.

Be this as it will, as we owe a great deal of deference to the accuracy of the great men who have made theſe experiments, we do not pretend intirely to diſallow them, only it were to be wiſhed, that experiments of this importance were more known than they are; in the mean time, till we have an occaſion of informing ourſelves hereof, or of making experiments of the like tenor ourſelves, we ſhall ſuppoſe any bounds of the air's condenſation as well as dilatation unknown, nor make any ſcruple for ſtretching them as far as we may have occaſion for, with this reſtriction, that our reaſoning ſhall be reduced to the terms of the experiment, whenever it appears certain and deciſive.

According to the experiment of M. *de la Hire*, related in M. *du Hamel's* hiſtory of the academy for the year 1696, a column of air, 37 fathoms 3 feet high, over the ſurface of the earth, only weighs as much as 3 lines $\frac{1}{6}$ of mercury, when preſſed with the weight of the atmoſphere, which was then 27 inches 5 lines $\frac{4}{6}$; but as this weight for diverſe reaſons does not always remain the ſame, but is ſometimes leſs and ſometimes more, we ſhall ſuppoſe, for the more eaſy calculating, that a column of air, 36 fathoms high, preſſed with 28 inches of mercury, weighs as much as 3 lines thereof, which amounts nearly to the ſame thing, tho' it comes nearer the truth, as will appear hereafter.

Suppoſe then with M. *de la Hire*, after the experiments of M. *Mariotte*, which we ourſelves have verified, that quantities of air, equal in weight, poſſeſs places, which are reciprocally proportional to the weights wherewith they are preſſed; on this principal, the weight of the air, which would fill the whole ſpace compreſſed by the terreſtrial globe, would be equal to a cylinder of mercury, whoſe baſe is equal to the ſurface of the earth, and whoſe height contains as many times 3 lines, as that ſpace contains orbs of equal weight with that of the 36 fathoms, wherein M. *de la Hire* made his experiment. Now the number of theſe orbs may be ſo great, that the cylinder ſhall vaſtly ſurpaſs the magnitude of the terreſtrial globe, as may be eaſily proved; for taking for inſtance the denſity of gold, which is known to be the heavieſt of all bodies, and to be no leſs than 14630 times heavier than air in our orb, it will be eaſy to gather, that this air will be reduced to the ſame denſity as gold, by a column of mercury, containing 14630 times 28 inches, that is, 409640 inches, ſince in this caſe, the bulks of air will be in the reciprocal ratio of the weights they are preſſed withal, according to M. *Mariotte*'s experiments and our own; and theſe 409640 inches will expreſs the height of the barometer in the orb, where the air was reduced to the ſame denſity as gold, and the number 2 lines $\frac{51632}{409640}$, the thickneſs to which the 36 fathoms of our orb would be reduced, that is, ſuch thickneſs of an orb in that part, would be as heavy as ours; ſo that 'tis evident all the other lower orbs of air, of the ſame thickneſs, would weigh conſiderably more than if they were mercury; now to learn the number of theſe orbs, we need only caſt an eye over the following table, which contains reductions of the thickneſs of ſeveral orbs of air, equal in weight with our own, by different heights of mercury, which in theſe orbs would be thoſe of the barometer, with their number and the depth they are to be below ours.

The orb on the ſurface of the earth, preſſed by 28 inches of mercury, being 36 fathoms thick.

The 32d orb, 992 fathoms below the firſt, being preſſed by 36 inches of mercury, will only be 28 fathoms thick.

The 68th orb, 1899 fathoms below the firſt, being preſſed by 45 inches of mercury, would only be 22 fathoms, 2 feet, 4 inches, 9 lines, $\frac{27}{45}$ thick.

The 136th orb, 3213 fathoms deep, preſſed by 62 inches of mercury, will only be 16 fathoms, 1 foot, 6 inches, 7 lines thick.

The 272d orb, 5026 fathoms below the firſt, being preſſed by 96 inches of mercury, would only be 10 fathom 3 feet thick.

The 544th orb, 7312 fathoms below the firſt, being preſſed by 164 inches of mercury, would only be 6 fathom, 10 inches, 6 lines $\frac{72}{164}$.

The 1092d orb, 9850 fathoms below the firſt, being preſſed by 301 inches of mercury, would be 3 fathoms, 2 feet, 1 inch, 1 line $\frac{119}{301}$.

The 2184th orb, 12580 fathoms below the firſt, being preſſed by 564 inches of mercury, would only be 1 fathom, 4 feet, 6 inches, 5 lines $\frac{114}{564}$.

The 4368th orb, 15492 fathoms below the firſt, being preſſed by 1120 inches of mercury, would only be 5 feet, 4 inches, 9 lines $\frac{672}{1120}$ thick.

The 8736th orb, 18404 fathoms below the firſt, being preſſed by 2212 inches of mercury, would only be 2 feet, 8 inches, 9 lines $\frac{1526}{2212}$ thick.

The 17472d orb, 21194 fathoms below the firſt, which is that where cork would be ſuſtained, being preſſed by 4396 inches of mercury, would only be 13 inches, 9 lines $\frac{1523}{4396}$ thick.

The 78960th orb, 28595 fathoms below the firſt, which is that where oil would be ſuſtain'd, being preſs'd by 19768 inches of mercury, would only be 3 inches, 8 lines, $\frac{11120}{19768}$ thick.

The 82208th orb, 28744 fathoms below the firſt, which is that where wax would float, being preſſed by 20580 inches of mercury, would only be 3 inches, 6 lines $\frac{6552}{20580}$ thick.

The 84112th orb, 28836 fathoms below the firſt, which is that where wine would be ſuſtain'd, being preſs'd by 21056 inches of mercury, would only be 3 inches, 5 lines $\frac{7616}{21056}$ thick.

The 86128th orb, 28929 fathoms below the firſt, which is that where water would be ſuſtain'd, being preſſed by 21560 inches of mercury, would only be 3 inches, 4 lines, $\frac{8512}{21560}$ thick.

The 124880th orb, 30408 fathoms below the firſt, which is that where honey would be ſuſtain'd, being preſſed by 31248 inches of mercury, would only be 2 inches, 3 lines $\frac{27216}{31248}$ thick.

The 638064th orb, 39910 fathoms below the firſt, which is that where tin would be ſuſtain'd, being preſſed by 159544 inches of mercury, would only be 5 lines $\frac{73192}{159544}$ thick.

The 689808th orb, 40208 fathoms below the firſt, which is, that where iron would be ſuſtain'd, being preſſed by 172480 inches of mercury, would only be 5 lines $\frac{8512}{172480}$ thick.

The 776048th orb, 40708 fathoms below the firſt, which is that where copper would be ſuſpended, being preſſed by 194040 inches of mercury, would only be 4 lines $\frac{24752}{194040}$ thick.

The 890960th orb, 41202 fathoms below the firſt, where ſilver would be ſuſtained, being preſſed by 222768 inches of mercury, would only be 3 lines $\frac{202608}{222768}$ thick.

The 991648th orb, 41551 fathoms below the firſt, where lead may be ſuſtain'd, being preſſed by 247940 inches of mercury, would only be 3 lines $\frac{127222}{247940}$ thick.

The 1172528th orb, 42181 fathoms below the firſt, which is that where mercury would be ſuſtained, being preſſed by 293160 inches of mercury, would only be 2 lines $\frac{284128}{293160}$ thick.

Laſtly the 1638448th orb, 43528 fathoms below the firſt, which is that where gold would be ſuſtained, being preſſed by 409640 inches of mercury, would only be 2 lines $\frac{51612}{409640}$ thick.

It muſt be obſerved, that tho' in this table, the orb, where mercury ſhould be ſuſtained, is ſaid to be 3 lines thick and upwards; yet, in reality, it muſt not be ſo much. The reaſon is, that the experiment upon which the calculus of this table is founded, were made by ſeveral perſons; ſome of whom found the weight of air to that of mercury, as 1 to 10800; and others, as 1 to 10470, in lieu of which ratios, it may be proper to take that of 1 to 10368, which is the mean between the other 2, and correſponds exactly to that of 3 lines to 36 fathoms; beſides, that this laſt is much more commodious for calculation. On this footing, the orb, where mercury would be ſuſtained, would only be the 1161104th its depth below ours 41931 fathoms, and the number of inches of mercury, it would be preſſed by 290276.

As to the ratios of air to other bodies expreſſed in this table, they were found from the ratio of 1 to 770, which is that found by M. *de la Hire* between the weights of air and water, and a table of weights given by M. *Blondel* in his mechanicks; that of cork was found by experiment, the heights of mercury expreſſed in the table being to each other, as the numbers which expreſs them divided by 28, we need not here give thoſe ratios, whereof theſe ſame numbers are the equimultiples.

You may now clearly perceive, that at 43528 fathoms depth, the air would be at leaſt $\frac{1}{4}$ heavier than mercury, we ſay at leaſt, by reaſon the depths aſſigned to theſe orbs, are rather too great than too little. Dr. *Halley*, in the place already quoted, ſhews, that theſe depths are repreſented by ſpaces, comprehended between a hyperbolical line, its aſymptote, and 2 perpendiculars to the aſymptote, repreſenting the productions of the ſame bulk of air by heights of mercury, whereof the part of the aſymptote, comprehended between ſuch perpendiculars, is the difference; whereas for the eaſier calculating, we have here ſuppoſed the hyperbolical ſide of this ſpace to be a right line, which gives thoſe depths ſomewhat greater than they ſhould be, though the difference is not very conſiderable.

Now this depth of 43528 fathoms is only the 4th part of the ſemi-diameter of the earth, which would contain many millions of millions more orbs, equal in weight with ours; ſuppoſing that the denſity of the air as not limited to that of the heavieſt bodies known among us beyond this depth, that vaſt ſphere of 6451538 fathoms in diameter ſtill remaining of the terreſtrial globe, may only be full of air prodigiouſly condenſed, and vaſtly heavier than the heavieſt bodies on the ſurface of the earth.

But we have ſhewn by the experiments related in the memoirs of 1702, that the more the air is compreſſed, the more will the ſame degree of heat increaſe the force of its ſpring, and render it capable of the greater effect; that the heat, for inſtance, of boiling water will increaſe the force of this ſpring beyond what it is in the degree of heat, which, in our climate, is called temperate, by a quantity equal to $\frac{1}{3}$ of the weight it is preſſed withal, which is enough to induce us to believe, that a degree of heat, which, in our orb, is only capable of a very moderate effect, becomes capable of a very violent one in the lower orbs; and as there are degrees of heat in nature, vaſtly greater than that of boiling water, 'tis poſſible, there may be ſome, whoſe violence being thus aſſiſted by the weight of the air, may be more than ſufficient to break and overturn this ſolid orb 43528 fathoms thick, wherein all the bodies we are acquainted of are contained, and whoſe weight, however enormous it may ſeem, is only a trifle in compariſon of the reſt.

But if it be obvious, what effect heat would produce in theſe lower orbs, we do not ſo eaſily conceive, how it could be communicated thither otherwiſe than by deſcending through the upper orbs, for want of other iſſues; ſince the air of theſe orbs being extremely condenſed, can only contain very few particles of fire in its intervals; and that as this condenſation grows exceſſive towards the centre of the earth, it muſt be almoſt wholly void of air in that part. This ſentiment, 'tis true, ſtands quite oppoſite to that of M. *Deſcartes*, and to the ſyſtem of the central fires;

but this alone ſhould not make it be diſcarded, till by other experiments, more convincing than thoſe we here go upon, it be ſhewn erroneous.

V. *Remarks on the table of the degrees of heat in the philoſophical tranſactions, for* April 1701, *read by M.* Geoffroy *in the aſſembly on* Tueſday, July 24, 1703. *by M.* Amontons *; *tranſlated by Mr.* Chambers.

As to the firſt article, we do not ſee why the author ſhould fix the firſt degrees of heat and cold to the moment when water begins to freeze, ſince there are other liquors which freeze more or leſs readily than water, and which might have had preference thereto; nor do we ſee, what could oblige him to make the diſtinction of hot and cold degrees, ſince every quantity of heat may be called either hot or cold, according as it is compared, either to extreme cold or intenſe heat. The degree of cold, for inſtance, where water congeals, being a conſiderable degree of heat, when compared to extreme cold; be this as it will, the degree of heat, which the author lays down as the firſt in his ſcale, is the ſame, as that marked on my thermometer for 51 inches, 6 lines; and that which he calls 34 degrees and $\frac{1}{2}$, which he ſays, boiling water cannot paſs, is that marked on my thermometer at 73 inches; ſo that if from theſe 73 inches, we ſubſtract 51 inches and $\frac{1}{2}$, the difference will be 21 inches $\frac{1}{2}$, which being divided by 34 $\frac{1}{2}$, will give 7 lines $\frac{21}{23}$ of my thermometer for each degree of this author. On this footing, what he calls the degree of heat of air in winter, would extend from 51 inches and $\frac{1}{2}$, to 52 inches, 8 lines $\frac{22}{2}$.

---

* Aug. 4, 1703.

What he calls the degree of heat of the air in ſpring and autumn, would extend from 52 inches, 8 lines $\frac{22}{23}$, to 53 inches, 11 lines $\frac{21}{23}$, and his temperate would be at 53 inches, 4 lines $\frac{10}{23}$, which is 7 lines $\frac{13}{23}$ below ours, that is, below the temperature of the vaults of the obſervatory.

What he calls the degree of heat of ſummer's air, extends from 53 inches, 11 lines $\frac{21}{23}$, to 55 inches, 10 lines $\frac{8}{23}$, which is 2 inches, 9 lines $\frac{15}{23}$ below our greateſt heat, and the whole compaſs which he allows the variations of air, between the heats of ſummer and winter, is only 3 inches, 8 lines $\frac{20}{23}$; whereas with us, it is found 5 inches, computing, as he has done, from the degree wherein water freezes, which is far from being the utmoſt degree of cold felt in our climate, any more than 56 inches, 6 lines, are the greateſt heat. The whole extent of this difference being uſually in the open air, and without ſun about 8 inches, which induces us to think, that the author made his obſervations in a cloſe place, and comparing his temperate with ours, it readily appears, that his obſervations were made in the colder climate. 'Twere to be wiſhed, the author had told us ſomething hereof, and had expreſſed the ſubterraneous temperature of the place of his obſervations.

The degree, which he calls the degree of heat of noon-tide air in the month of *July*, and which probably determined with him the heat of the air in the ſummer's ſolſtice, is much below that found here, his only reaching to 55 inches, 10 lines $\frac{8}{23}$, and ours to upwards of 58 inches.

The degree, which he calls the greateſt degree of heat, that the thermometer is capable of receiving from the natural heat of the human body, correſponds to 58 inches, 11 lines $\frac{2}{23}$ of my thermometer; and the experiments I have made on this head, would convince me he had determined it very accurately, if it were not certain, that this natural heat is always the ſame both in ſummer and winter.

When the thermometer ſtood at 29 inches, 5 lines, ſeveral perſons, whoſe pulſes were as undermentioned, made the thermometer riſe by the heat of their hands, to the heights expreſſed in the other column.

Pulſes in a minute.	*Heights of the thermom.*
70	58 inch. 6 lin.
74	58—7
70	58—5
66	58—9
56	58—7
68	58—2
60	58—5
80	58—6

One of theſe perſons putting the thermometer in his mouth, could not make it riſe higher than by the heat of his hand.——By the way, it may be obſerved, that the pulſation of the arteries bears no relation to the natural heat, and that the one cannot be eſtimated from the other.

As to the 7th and 8th article, whoſe degrees of heat, as they are marked, correſpond, *viz.* the 7th to 60 inches, 8 lines of my thermometer, and the 8th to 62 inches, 1 line; it doth not appear, that any thing certain can be determined thereon, by reaſon that all men are not equally ſenſible, and I have had much ado to keep my hand any time in the bath of the 7th article, when my ſervants ſupported that of the eighth. For a conſiderable while the degree of heat, where the bath neither appeared to me hot nor cold, was 58 inches, 5 lines, which is preciſely that to which the heat of my hand would make the thermometer riſe; the thermometer then ſtanding at 56 inches.

The degree of heat of a bath, wherein melted wax poured therein begins to fix and looſe its tranſparence, was found the ſame as the author has repreſented it, and correſponds to 64 inches, 1 line, with my thermometer.——— The degree of heat of a bath, wherein he ſays a piece of wax will melt, anſwers to 66 inches, 5 lines $\frac{1}{2}\frac{1}{3}$ of my thermometer; but in my experiment, a piece of white wax, 10 or 12 grains heavy, would not melt intirely in water lower than 67 inches, 3 lines.——— A piece of tallow, of the ſame weight, melted at 61 inches, 10 lines, and a piece of butter at 59 inches, 9 lines.

As to the 2d column of his table, which contains the ſame degrees of heat in a geometrical expreſſion, it ſeems to be founded upon a falſe principle, which is, that water beginning to freeze has no degree of heat; whereas we find ſeveral other bodies, whoſe heat ſuffices to maintain their fluidity all that time; and thus ſo far is the heat of boiling water from being almoſt triple of the natural heat, or the degree of fuſion of wax double thereof; that 'tis more probable thoſe degrees of heat are only to each other, as the numbers 59 $\frac{1}{2}$, 66 $\frac{2}{3}$, and 73, which expreſs the force of the ſpring of the air, occaſioned by thoſe degrees of heat, when it has not liberty to extend, but is preſſed in the boiling water with 73 inches of mercury.

What the author ſays of the hot iron, wherewith he found the degrees of heat, which could not be had by the thermometer, is not very intelligible: his words are, *Calor quem ferrum ca-*

lefactum, corporibus frigidis ſibi contiguis, dato tempore communicat; *hoc eſt, calor, quem ferrum dato tempore mittit, eſt ut calor totus ferri*; which is certainly to be underſtood of the heat remaining; and all we can conceive to be meant by it is, that the quantity of heat it communicates in a certain time, is equal to what is left; ſo that a hot iron, which, in a certain time, had loſt $\frac{1}{2}$ its heat, would only looſe $\frac{1}{2}$ of that $\frac{1}{2}$, that is a $\frac{1}{4}$ in a ſecond time, equal to the firſt; an 8th in a third time; a 16th in a 4th time, and ſo of the reſt: but this reaſoning ſeems to ſuppoſe, without any grounds, that the ratio of two to one reigns continually in this decreaſing progreſſion, whereas any other ratio, as of 3 to 1, or 4 to 1, may equally obtain, according as the air which ſurrounds the iron, and to which it communicates its heat, is more or leſs cold; or as the iron is more or leſs hot, and the times of cooling longer or ſhorter; all which circumſtances may be diverſified to infinity, and will occaſion a like diverſity in the ratios of the progreſſion, whoſe terms are to expreſs the different degrees of heat. So that to make uſe of this method to advantage, we ſhould have as many tables of logarithms, as there may be different geometrical progreſſions; add that we are not afraid that the air, which continually ſucceeds around the hot iron all the time of its cooling, is of the ſame temperature, not to mention the neceſſity of ſuſtaining this hot iron by ſome body, to which it will communicate more or leſs of its heat, as ſuch body is more or leſs cold, or its maſs greater or ſmaller.——— Being unable, therefore, to determine any thing preciſely in this manner, I made uſe of another.

I put a bar of iron 59 inches long, and weighing 30 *lb.* almoſt upright upon charcoal in a furnace, where there might be the value of a buſhel, which done, I lighted the charcoal, and took care to add the quantity of another buſhel, and two ſeveral times in proportion, as the other waſted; and when the iron was heated, ſo that the lower end was quite white hot, it ceaſed to be red at the diſtance of 5 or 6 inches, and at the diſtance of 42 inches, was juſt ſufficient to put butter in fuſion. Upon this I laid it down horizontally, with the hot end on the fire, and the other ſupported by a piece of wood, and laying the following matters on it as carefully as I could, I found that thin glaſs would melt at 4 inches ½ from the lower end.

Lead at 8 inches ½; gunpowder kindled in the ſame place.

Tin melted at 11 inches.

Solder made of three parts of lead, two of tin, at 12 inches.

Drops of water boiled at 22 inches.

Virgin wax melted, at 30 inches 8 lines.

Tallow at 39 inches.

And butter, as already mentioned, at 42 inches.

Now if we conſider that the ſpaces on this bar, comprehended between the boiling water, the fuſion of wax, tallow, and that of butter, are to each other as the ſpaces marked on my thermometer, between the ſame degrees of heat; it will eaſily appear, how all the other degrees of heat, found by means of the bar, may be reduced into degrees of my thermometer. This I have effected in the following table, where on the one ſide are found all the degrees of heat, which I have learned by my own experiments; and on the other, thoſe delivered in the philoſophical tranſactions, both of them reduced into degrees of my thermometer, that they may be the eaſier compar'd together.

All that here remains, is to examine what the author has ſaid of the rarefactions of air, linſeed oil, and ſpirit of wine, with regard to which he ſeems to have been groſly overſeen, at leaſt there is a wide difference between us on this head, as well as in the experiments with the red-hot iron. The rarefaction of air, ſays he, under an equal degree of heat, was ten times greater than the rarefaction of the oil; and the rarefaction of oil, almoſt fifteen times greater than that of ſpirit of wine.

On this footing the rarefaction of air ſhould be 150 times greater than that of ſpirit of wine, under the ſame degree of heat, which I have found true by experiment; for in my air thermometer, its bulk at the time when water freezes, is to its bulk in boiling water, as 148 $\frac{28}{43}$, to 149 $\frac{28}{43}$, in the author's thermometer, made with linſeed oil. Theſe bulks are to each other, as 10000 to 10705, or as 14 to 15; and in my ſpirit of wine thermometer, theſe ſame bulks are to each other as 472 to 515, or as 11 to 12; now theſe augmentations of bulk; $\frac{1}{148}$, $\frac{1}{14}$, $\frac{1}{11}$, are to each other as the numbers 77, 814, and 1036, where we find, that inſtead of the air's rarefaction, under an equal degree of heat, being ten times greater than that of linſeed oil, it appears by this experiment, 10 times and $\frac{1}{2}$ ſmaller, and 14 times ſmaller than that of ſpirit of wine, which is far from being 150 times greater than that of ſpirit of wine; 'tis true, the author does not tell us in what manner he obſerved this great rarefaction of air, and that in my experiment, with the air thermometer, the air is not only continually preſſed with the weight of the atmoſphere, but in the time of freezing, compreſſed likewiſe with a column of mercury, 23 inches and

$\frac{1}{2}$

½ high, which in all amounts to 51 inches and ½; and that this column is continually increaſing, ſo that when the water boils, the column is 73 inches high; but ſuppoſing the air were only preſſed by the weight of the atmoſphere, its bulk, according to M. *Mariotte*'s experiment, could only be increas'd in the ratio of 103 to 146; in which caſe, our rarefactions would be to each other as the number 4757, 814, 1036; where it may be obſerved, that the rarefaction of air, under an equal heat, can at moſt only be 5 times greater than that of ſpirit of wine. As to the rarefaction of linſeed oil, ſo far is it from being 15 times greater than that of ſpirit of wine, that it evidently appears, by the above experiments, to be leſs than that of ſpirit of wine, or even of brandy, for our ſpirit was no more in the ratio of 814 to 1036.

A ſcale of ſeveral degrees of heat, found both by the reſiſtance of the thermometer, and of red-hot iron, compared with thoſe in the philoſophical tranſactions, for *April* 1701, both the one and the other, expreſs'd by the number of inches and lines of mercury high, which ſuch degrees of heat would enable the air incloſed in a glaſs thermometer to ſuſtain; ſuppoſing this height ſuſtain'd in boiling water, preſſed by the atmoſphere, to be 73 inches.

Degrees

Degrees of heat, taken from the philoſ. tranſact.			Degrees of heat, found by experiment.	
inch.	*lin.*		*inch.*	*lin.*
51	6	Degree of heat of winter's air, when water begins to freeze.	51	6
51	6	Degrees of heat of winter's air.	51	6
52	2		52	4
52	9		53	2
52	9	Degrees of heat of vernal and autumnal air.	53	2
53	4		54	0
54	0		54	10
54	0	Degrees of heat of ſummer air.	54	10
54	7		55	8
55	2		56	6
55	2	Degree of heat of the air, in the middle of *July*.	56	6
59	0	The greateſt degree of heat which the thermometer can receive from the heat of the human body.	58	9
59	0	That of a bird brooding on its eggs.		
60	8	The greateſt heat of a bath, which the hand is able to endure, by keeping it continually ſtirring.		
62	1	The greateſt heat of a bath, which the hand can ſuſtain for any continuance, without ſtirring it about.		
64	1	Heat of a bath, wherein melted wax caſt in it begins to fix and loſe its tranſparence.	64	1

	Degree of heat of the bath, wherein my hand neither felt heat nor cold.	58 — 5
66 — 5	Degree of heat of a bath, wherein a piece of wax, remains in fuſion without ebullition.	67 — 3
	Degree of heat of a bath, wherein a piece of tallow melts.	61 — 10
	Degree of heat of a bath, wherein a piece of butter melts.	59 — 9
73 —	Degree of heat which makes water boil, and beyond which it cannot go.	73 —
73 — 0	Degree of heat, wherein a mixture of 2 parts of lead, 3 of tin, and 5 of biſmuth fuſed, begin to harden again.	
73 — 4 or 74 — 0	Degree of heat, wherein red-hot iron cooling, ceaſes to make drops of hot water, thrown thereon, boil.	
74 — 6	Degree of heat, wherein the ſame red-hot iron ceaſes to make drops of cold water boil.	
	The ſmalleſt degree of heat, wherein a mix-	

ture of 1 part of lead,
4 of tin, and 5 of biſ-
muth, melt and remain
76— 7 fluid.

The ſmalleſt degree
of heat, wherein a mix-
ture of equal parts of
81— 5 tin and biſmuth melt.
to This mixture fixes as
80—10 it cools.

The degree of heat
capable of melting a
mixture of 2 parts of
tin, and 1 of biſmuth;
as well as a mixture of
3 parts of tin, and 2 of
lead; and wherein the
mixtures of 5 parts of
tin and 2 of biſmuth,
and of equal parts of
87— 0 lead and biſmuth, fix.

The ſmalleſt degree
of heat, wherein a mix-
ture of 1 part of biſ-
muth, and 8 parts of
93—10 tin, is capable of fuſing.
96— 4 The degree of heat,
to which puts tin in fu-
95— 2 ſion. 80— 3

The degree of heat
which melts biſmuth;
as alſo the mixture of
4 parts of lead and 1
of tin; and wherein the
mixture of 5 parts of
lead and 1 of tin, fixes
102— 0 again.

111— 4	The ſmalleſt degree of heat, which melts lead.	82—
	The degree of heat, which puts glaſs in fuſion.	84— 7
	The ſame is the degree of heat of red-hot iron.	
	The heat of a large charcoal fire, which turned iron extremely red, and made it ſcale.	87— 7
	Gun-powder took fire with the ſame degree of heat as melted lead.	82—
	Solder made of 3 parts of lead and 2 of tin melted, at	79— 1
122— 6	The degree of heat, wherein burnt bodies ceaſe ſhining in the dark, and wherein heated bodies begin to yield a little light, but ſo weak, as hardly to be perceivable.	
	The ſame degree of heat will melt a mixture of equal parts of tin and regulus martis.	
	At the ſame degree of heat, a mixture of 7 parts of biſmuth, and 4 parts of regulus martis melted together, began to fix.	

136—3	The degree of heat, wherein burnt bodies afford light in the dark, but none in the crepuſculum.
	At this degree of heat, the mixture of 2 parts of regulus martis, and 1 part of biſmuth melted together, began to fix.
	The ſame befel a mixture of 5 parts of regulus martis and 1 of tin.
142—6	The degree of heat, wherein melted regulus martis fixes.
151—10	The degree of heat, wherein burnt bodies afford light in the crepuſculum immediately after the ſun's riſing or ſetting, and none, or very little in the open day.
171—2	The degree of heat of a little ſea-coal fire without blowing, as alſo the heat of iron turned as red as it could be in this fire.
176—1 to 181—4	Degrees of heat of a moderate wood fire.

VI.

VI. *Barometrical experiments made upon ſeveral mountains of* France, *by M.* Maraldi*.

In a journey that we took with Meſſ. *Caſſini*, *Chazelles*, and *Couplet*, under the direction of M. *Caſſini*, for the determination of the meridian, we made ſome barometrical experiments on ſeveral mountains of *Auvergne*, *Languedoc*, and *Rouſſillon*, the heights of which above the ſurface of the ſea, we meaſured geometrically. Theſe new experiments, which were made at much greater heights than any that were taken before, may ſerve to ſhew the properties and extent of the air, and how much it is rarefied at different heights from the ſurface of the earth.

We ſhall not here relate the particulars of the operations and calculations, which we were obliged to make, in order to find the height of theſe mountains. It will be ſufficient to ſay, that having determined their diſtances, with regard to the points contained in the triangles of the meridian, we have at the ſame time obſerved their apparent heights, with regard to each other, and that we have made theſe operations by a ſeries of uninterrupted obſervations from the mountains of *Berri* to the ſea-coaſt of *Rouſſillon*. Then having obſerved the apparent height of ſome of the ſame mountains, and knowing their diſtance with the ſemi-diameter of the earth, we determined the perpendicular height above the level of the ſea; and we concluded the perpendicular height of thoſe which we did not ſee from the ſea-coaſt, by the difference of the heights which had been obſerved between theſe mountains, and the reſt, which had been already known.

---

* Nov. 14, 1703.

To follow ſome order in the report of theſe experiments, we ſhall begin with the compariſon of the obſervations made on the barometer at the ſame time, at *Colioure*, a town of *Rouſſillon*, and at the obſervatory, in the weſtern tower of the great hall. This compariſon ſerves to know the different height at which the barometer keeps at the ſame time in theſe two different places, to determine the height of the obſervatory above the ſurface of the *Mediterranean*; and conſequently to know by the obſervations made at the obſervatory, what the height of the quickſilver was at the ſea-coaſt during the time of our making the obſervations on the mountains, when we could not make them at the ſame time at the ſea-coaſt.

During our ſtay at *Colioure*, from *Feb.* 19, to *March* 12, of the year 1701, to make the neceſſary obſervations for the meridian, we placed the barometer in a houſe, ſituated on a rock by the ſea-ſide, in a place that was elevated 69 feet above the level of the ſea. By the compariſon of theſe obſervations made during one month, we find, that at *Colioure* the quickſilver was uſually 3 lines $\frac{1}{3}$ higher than at the obſervatory. If we ſuppoſe the height of the air, which agrees to a line of quickſilver at the ſea-ſide, to be 10 toiſes, as M. *Mariotte* ſuppoſes it, in his ſecond eſſay on the nature of the air, by the difference of the height of the quickſilver, obſerved at the ſame time at the obſervatory and at *Colioure*, of 3 lines and $\frac{1}{3}$, we ſhall have the great hall of the obſervatory more elevated than *Colioure*, by 33 toiſes and $\frac{1}{3}$; which being added to 11 toiſes $\frac{1}{2}$ difference of height, between the level of the ſea, and the place where the barometer was obſerved, make the great hall of the obſervatory 45 toiſes higher than the *Mediterranean*, and the height of the quickſilver leſs by 4 lines ⅙ at the obſervatory, than at the ſea-ſide. This height of the hall of the obſervatory differs but one toiſe from that which M. *Picard* has determined of the level of the ocean, which thereby appears to be the ſame level with that of the *Mediterranean*.

After theſe obſervations made at *Colioure*, we carried the barometer upon a ſummit of the mountains near *Rouſſillon*, called *la Meſſane*, the height of which above the ſurface of the ſea was meaſured geometrically 408 toiſes. On this mountain we found the height of the barometer 25 inches, 5 lines. We had found it ſome hours before at *Colioure* juſt 28 inches. The difference is 2 inches, 7 lines; to which, if we add 1 line ⅙, for the height of the place where the barometer was, we ſhall have 32 lines diminution of the quickſilver for the height of 408 toiſes.

We made another experiment with the barometer on the top of *Bugarach*, a mountain of *Languedoc*, the height of which above the level of the ſea, has been determined three different ways to be 648 toiſes. The barometer on the top of the mountain held ſuſpended at 23 inches, 8 lines ⅓, at the ſame time that it held at the obſervatory at 27 inches, 3 lines; to which, if we add 4 lines ½, which are due to the height of the obſervatory, above the level of the ſea, we ſhall have the height of the quickſilver reduced to the ſame level of 27 inches, 7 lines; the difference of which, from 23 inches, 8 lines ½, is 46 lines ½, which anſwer the height of 648 toiſes.

In *October*, we obſerved on the top of the mountain of *la Coſta*, near the *Mont-d'Or*, in *Auvergne*, the height of the quickſilver to be

23 inches, 4 lines. It was obſerved the ſame day at *Paris* to be 27 inches, 10 lines, the difference of which is 4 inches, 6 lines; to which, if we add 4 lines $\frac{1}{6}$, due to the height of the obſervatory above the level of the ſea, we ſhall have 4 inches, 10 lines, for 850 toiſes, which the top of this mountain is elevated above the ſurface of the ſea.

From *la Coſta*, and ſeveral other points of the meridian, we ſee the *Puy Domme*, a famous mountain near *Clermont*, in *Auvergne*, by the barometrical experiment, which M. *Perier* made on its ſummit; which is alſo related in M. *Paſchal*'s *Traité de l'Equilibre des Liqueurs*. This obſervation, which is the firſt that was known to be made on this ſubject, and is conſiderable for the great variation of the quickſilver, from the foot of the mountain to the ſummit, was indeed ſufficient to prove, as was propoſed, that the preſſure and weight of the air, are the cauſe of the ſuſpenſion of the quickſilver; but it would ſerve but imperfectly to find the height of the atmoſphere, as has ſince been attempted, becauſe the height of the mountain above *Clermont*, was determined only by gueſs, and its height above the ſurface of the ſea was entirely unknown. By the ſame method that we have uſed to find the height of other mountains, we have ſupplied the principal circumſtance that was wanting in this obſervation, having determined its height above the level of the ſea, to be 810 toiſes, 40 toiſes lower than the mountain of *la Coſta*. By the compariſon which we have made of M. *Perier*'s experiments, having had regard to the different height of the quickſilver between *Paris* and *Clermont*, which reſult from the obſervations made a year after that of *Puy Domme*, and to the falling of the quickſilver from *Clermont*, quite to the top of the mountain, we find that at the height of 810 toiſes, which is the height of *Domme*, as we meaſured it, there would be a diminution of 4 inches, 11 lines, with regard to the ſurface of the ſea, which would be a line of diminution more than we found upon *la Coſta*.

This difference may come from the reductions which we are obliged to make, as there were no obſervation made at *Paris* on the day of the obſervations, which ſhould have been, becauſe of the variation which often happens in the height of the quickſilver from one day to another; and indeed it may come from the difficulty of being always exact in ſuch nice experiments.

The obſervations, which we have related, added to thoſe which were made in 1672, by M. *Caſſini* at *Notre Dame de la Garde*, near *Marſeilles*, and to thoſe which M. *de la Hire* made 10 years afterwards upon mount *Clairet*, near *Toulon*, have ſerved us to find a rule, according to which the quickſilver diminiſhes at different heights of the air above the ſurface of the ſea, which agrees very well with the obſervations, and is eaſily retained.

Suppoſe, that at the ſea-coaſt the height of the air, agreeing with a line of quickſilver, is 61 feet, which make 10 toiſes, 1 foot, within $\frac{1}{60}$ part of M. *Mariotte*'s hypotheſis; that the height agreeing with the ſecond line, is 1 foot more than the firſt; the third a foot more than the ſecond, and ſo on.

According to this hypotheſis, from the ſurface of the ſea, to the height of 178 toiſes, the quickſilver ſhould fall 15 lines $\frac{2}{3}$. M. *Caſſini*'s obſervation, which was made at this height, gives 16 lines $\frac{1}{3}$. At the height of 257 toiſes, the hypotheſis makes the diminution of the quickſilver 21 lines $\frac{8}{9}$. M. *de la Hire*'s obſervation makes it 21 lines $\frac{1}{2}$. At the height of 408 toiſes, according to the calculation, the quickſilver ſhould fall 32 lines $\frac{1}{6}$, which agrees with the obſervation of *la Maſſane*. At the height of 648 toiſes, the hypotheſis makes the falling of the quickſilver 46 lines $\frac{1}{7}$, inſtead of 46 lines $\frac{1}{2}$, according to the obſervation of *Bugarach*. At the height of 851 toiſes, it is 57 lines $\frac{7}{8}$, inſtead of 57 $\frac{5}{6}$, which reſults from the obſervation of *la Coſta*.

By this rule, which repreſents all the obſervations hitherto made within leſs than a line, we may know within a few toiſes, by the obſervation of the barometer, the height of a mountain, the elevation of the places of the earth above the ſurface of the ſea, tho' at a great diſtance from it, and how far one place is elevated above another, to the height of near half a league above the ſurface of the ſea, which is the height where our obſervations end.

Following the ſame principles, and ſuppoſing the height of the quickſilver at the ſea-coaſt to be 28 inches, as it commonly is, we have calculated what the height of the atmoſphere ſhould be, in this manner. If from the height of the quickſilver, which, at the ſea-ſide, is 28 inches, or 336 lines, you ſubſtract the half, which is 168, and multiply it by 337, the number of lines of the ſuſpenſion of the quickſilver augmented by unity, and divide the product by 6, the quotient will be 9436 toiſes; which being added to 3360 toiſes, due to 336 lines in the proportion of 10 toiſes for each, there will be 12796 toiſes, which make 6 leagues $\frac{1}{2}$ for the height of the atmoſphere. If we calculate in like manner the height of the laſt line but one, we ſhall find that the air, which anſwers to the greateſt height, would there be above 6 times more rarefied, than the air is at the ſea-ſide.

We do not pretend to give for the exact height of the atmoſphere, that which we have juſt found by the calculation; it is an attempt that we have been willing to make, to ſee what reſults from our obſervations made at much greater heights, and in much greater number than what we had before.

We know how difficult it is to make a juſt concluſion of the meaſure which we have made of the reſt of the atmoſphere, which is incomparably greater, even though the upper air ſhould be of the ſame conſtitution with that which is near the earth. But beſides theſe difficulties, there are alſo other conſiderable ones, there are others which may come from the variations which happen to the barometer in the ſame country, and from the different variation, which is found in different climates.

For we have obſerved, that the weight of the air varies conſiderably in the ſame places at different times; that it is uſually more heavy in fair ſerene weather, and lighter when it is cloudy and full of vapours; which ſeems ſo oppoſite to the judgment that we naturally make of it, that before theſe experiments ſome celebrated philoſophers made no difficulty to ſuppoſe the contrary.

A great number of experiments made ſome time ago in *Spain*, *Italy*, and *England*, and compared with thoſe which we made at the ſame time at the obſervatory, have ſhewn, that the barometer varies in the ſame circumſtances of time; and what is very conſiderable, theſe variations happen moſt commonly on the ſame days, eſpecially thoſe which are quick and ſudden. We have found, that the variations of the barometer are greater in the northern than in the ſouthern countries. We have obſerved, that in *Sweden* they are $\frac{1}{13}$ of the greateſt height of the barometer; that they are greater there than in *France*, where they are but one $\frac{1}{17}$; that in *France* they are alſo greater than between the tropicks, and toward the equinoctial, where they do not arrive to $\frac{1}{50}$ part. We have alſo obſerved, that the barometer, ſituated at a little height above the ſurface of the ſea, always remained lower in the obſervations made near the equinoctial, than in *Europe*; ſo that if we ſuppoſe the height of the air above the ſurface of the quickſilver to be proportioned to its ſuſpenſion in the barometer, the height of the atmoſphere would be greater towards the N. pole, than near the equator.

To obtain more certain informations concerning the extent of the air by experiments made at great heights, we know none more proper than ſuch as ſhould be made on the *Canigou*, which is the moſt ſouthern mountain of the *Pyrenæans*, where the triangles of the meridian are terminated. It is higher than the mountains of *Auvergne*, *Languedoc*, and the *Pyrenæans*, which we have obſerved. It is alſo nearer to the ſea-coaſt, from whence it is ſeen, not being above 10 leagues from it; ſo that the experiment might be made at the ſame time on the ſea-coaſt and on the mountain, without having occaſion for any reduction. The height of the *Canigou*, above the ſurface of the ſea, meaſured 2 different ways, has been found to be 1440 toiſes, which are a little leſs than $\frac{3}{4}$ of a league in perpendicular height, which, according to the hypotheſis, would give 7 inches of diminution, which make $\frac{1}{4}$ of the greateſt elevation of the barometer.

rometer. We had determined this height, as well as that of ſeveral other mountains, in the deſign of making barometrical experiments upon it; but we were not able to execute it, becauſe of the great quantity of ſnow which covered them at the time of our obſervations.

As the principal difficulty in theſe experiments conſiſts in knowing the height of the places where they are made, we have thought it proper to give here a catalogue of the principal mountains, of which we have found the height above the ſurface of the ſea, to give an opportunity to the learned near theſe mountains, to make barometrical experiments upon them, and to ſee if the hypotheſis propoſed anſwers to their obſervations. Ir would be of uſe alſo, to make upon ſome of theſe mountains, ſome obſervations for the refraction of the ſtars. The experiments which M. *Caſſini* had made at *Marſeilles*, and agree with thoſe which we made laſt, having given the height of the air much greater than that which reſults from the hypotheſis, which he had uſed in determining the aſtronomical refractions, gave him room to conjecture, that there might be ſome fluid matter diſperſed in the lower part of the air, and a little elevated above the higheſt mountains of the earth, which was the principal cauſe of the refractions of the ſtars. Theſe obſervations made both at the ſea-ſide, and on the higheſt mountains, would ſerve to know whether this refractive matter, different from that of the air, is really in nature; whereas at preſent he propoſes it only as a commodious invention for the calculation of the refractions.

A catalogue of the principal mountains, of which we have meaſured the height above the ſurface of the ſea.

	Toiſes.
The *Maſſane* in *Rouſſillon*	408
Bugarach in *Languedoc*	648

Mountains of Auvergne.

The *Puy Domme*	810
The *Coſta*	851
The *Puy de Violent*	853
The *Cantal*	984
The *Mont d'Or*, which is the higheſt mountain of *Auvergne*	1030
The *Mont Ventoux*, in the *Comtat* of *Avignon*	1036

Mountains of the Pyrenæans.

The mountain of *S. Bartholomew*, in the country of *Foix*	1185
The mountain of *Mouſſet*	1258
The *Canigou*	1440

VII. *Obſervations on a petrified brain, by M.* du Verney, *jun.* *.

Almoſt three months ago an ox broke looſe four times, juſt as the butcher was going to knock him down. This remark is not uſeleſs, becauſe it ſhews, that it was no weak and faint animal; but on the contrary, very ſtrong and vigorous. When they came at laſt to open his head, the brain reſiſted the cleaver. The butcher thinking he had ſtruck againſt the iron ring, to which the head was faſtened, redoubled his blow, but to no purpoſe; and having in vain ſtruck a third and fourth time, he was obliged to take a hammer, and break the skull in pieces to get out the brain. After he had maſhed the ſkull with his hammer, he and his companions were ſurpriſed to find a great ſtone inſtead of a brain. They brought it to me; and I was ſtill more ſurpriſed than they, when I ſaw this prodigy. As all the parts of the ſkull had been broken in pieces, it was impoſſible for me to replace every part.

---

* Nov. 14, 1703.

The figure of this brain is ſingular, and very different from that of a common brain. Its ſurface is all over uneven and rugged; not much unlike a piece of rock-work. The two great lobes are thicker and cloſer than they are naturally. Each of them has a proceſs at the upper part; that of the left lobe, which proceeds about a finger's breadth beyond the ſurface of the reſt, is rounded; that of the right lobe, which riſes about an inch, is larger, and forms a ſort of ſaddle, terminated at each extremity by a pummel, of which the hinder one is round like a little button, and the other is much bigger and broader, and has a ſort of groove in the middle. Theſe two eminences leave a conſiderable ſpace between them, which is continued down below the baſe of the ſkull, forming from ſpace to ſpace unequal cavities, of a greater or leſs bigneſs, of which the two more conſiderable ones may contain a ſmall filberd. The lower part of the left lobe makes a blunt proceſs, which advances beyond that of the right-ſide. The different windings, which form the furrows of the brain, ſeem in ſome places larger than uſual, and alſo ſeparated, leaving ſinuoſities and depreſſions, which probably were hollowed by the motion of the veſſels, which were there preſerved; and alſo on the interior *lamina* of the ſkull, there are furrows formed by the courſe of the veſſels. In ſeveral places of the lobes of this brain, there are windings alſo, which are ſtill joined by the *pia mater*, which is hardened and petrified almoſt every where elſe, as well as the brain; only within the hinder part of the lobes, which joins to the lateral part of the *cerebellum*, there is alſo a tender and ſpongy ſubſtance, which was not hardened and petrified like all the reſt, which is eaſily felt by introducing a probe; for this part does not reſiſt the probe like the reſt; and that place is properly what is called the third cavity, or the third ventricle, above which the pineal gland is ſituated, which is at the meeting of the *ſinus's* of the *dura mater*. Tho' the eminence in this place is bigger than a walnut, its ſituation gives room to believe, that it is the pineal gland. This gland is in a manner articulated by its anterior and ſuperior part, by the two proceſſes of the lobes which we have deſcribed; and by its poſterior and inferior part with the *cerebellum*. At the lower part of this gland there are ſeveral cavities, the moſt conſiderable of which, at the ſuperior and poſterior part of the right lobe, forms a great ſinuoſity, which paſſing over the right-ſide of the *cerebellum*, terminates in a great cavity, about a finger's breadth in diameter, where the beginning of the *medulla oblongata* muſt be. Above the ſame place there is alſo a ſlight depreſſion, about the ſize of a *denier*, which depreſſion is alſo covered with its membrane, and appears tranſparent.

At the baſe of the skull, and by the ſide of this cavity, near to another pretty conſiderable ſinuoſity, there is alſo a tender, ſpongy ſubſtance, like marrow; for in introducing a probe, it not only penetrates very eaſily to the depth of 5 or 6 lines, but in drawing it back, we find it all covered with this marrow; and what is very ſurpriſing, is, that the neighbouring parts are all as hard as marble.

The *cerebellum* is ſituated obliquely, riſing forwards upon the left lobe; and the *proceſſus vermiformis*, which proceſs is naturally bent back and hid under the lobes, is here found elevated above the left lobe. Between the ſame lobe and the ſame proceſs there is a conſiderable cavity, of an irregular figure, approaching to an oval, which penetrates very forward into the ſubſtance of the brain, and at the place where it ends, ſeems leſs hard, and only oſſified. The *cerebellum* has almoſt preſerved its natural figure, and there is no conſiderable cavity found in it, but only ſmall depreſſions and *ſinus's*, which ſeem to have been formed by the ſeparation of the furrows.

In the baſe of the brain, which was cut by the butcher, we may ſee the grey and the white part diſtinctly, tho' they are quite petrified.

After the circumſtances, which I have juſt related, I believe no one can queſtion, that this brain really belonged to a living animal. If it had been all ſtone, it might have been ſaid to be a production like thoſe bones, which ſome take for foſſils, and to theſe wonderful petrifications, which are ſometimes found in the earth, and are, if I may ſo ſpeak, ſports of nature. But in this petrified brain ſome parts are bony; and there is ſtill a tender and ſpongy ſubſtance at its baſe; there is alſo a ſubſtance in it like marrow, with which the probe, that has been introduced, is covered. All theſe facts prove invincibly,

cibly, that this brain is no production of chance or art, as thoſe foſſil bones and ſome artificial productions may be. The ſingularities in this brain, are characters of truth, which chance cannot imitate, and art knows not how to imitate.

Tho' examples are not neceſſary to authoriſe a thing ſo evident; I have been delighted to find in the works of a celebrated author, a fact like this, which confirms it; I mean the famous *Bartholin*, who relates it in his *anatomical centuries* *. He ſays, that in his time, in *Sweden*, as a butcher was going to kill an ox, and had ſplit his ſkull, the brain reſiſted the blow, and made the cleaver fly back; that the maſs of this brain was found to be petrified; that the ox always hung his head down when alive; that he grew very lean, which determined his maſter to ſell him to the butcher. *Bartholin* adds, that he had not ſeen this brain; but that *Steno Bielke*, the *Swediſh* ambaſſador, from whom he had received the information, had aſſured him that he had ſeen it in Count *Oxenſtiern*'s houſe, where it was kept, and that this ambaſſador had promiſed him to ſend a very exact figure, and a piece of it. The difference between our obſervation and that of *Bartholin*, is, that the ox, which he ſpeaks of, was extremely lean, whereas ours was fat, and in good condition.

But how ſhall we anſwer the objections, which theſe obſervations furniſh againſt what has always been thought on ſo important a ſubject? *Bartholin* confeſſes himſelf very much puzzled by it. *The brain*, ſays he, *muſt no longer be reckoned among the noble parts, ſince its functions are not abſolutely neceſſary to life.* Thus is the brain degraded from the nobility, which it has hitherto enjoyed. I agree with this author, that the affair is very puzzling; but yet upon due conſideration, I believe we can give ſome ſolutions to theſe difficulties.

---

* Lib. VI. Hiſt. 91.

If all the parts of our ox's brain were equally petrified, and all over as hard as they are in a great number of places, it would be very difficult, not to ſay impoſſible, to explain how the animal could have lived; the communication of the brain with all the other parts of the brain being intercepted, and the ſpirits, which it muſt continually ſend for the nouriſhment and enlivening of the parts, not being any longer able to paſs. But the ſoft and ſpongy ſubſtance, which is alſo found in ſome places, as has been obſerved, gives light into the eſtabliſhment of ſome probable conjectures. We may ſay, that this marrow-like ſubſtance, has always been able to furniſh a certain quantity of ſpirits, not only to cauſe the nerves of the brain to perform their ordinary functions; but alſo to ſerve for the nutrition of all the other parts. And what gives room to believe it, is, that in the baſe of the ſkull, there were alſo found ſome nerves, which ſeemed to be in their natural ſtate, as the ſpinal marrow alſo was. We cannot queſtion but this was ſufficient for all theſe different functions, if we conſider, that ſome perſons have been ſeen, who have had the ſubſtance of their brain very much injured, even to the baſe of the ſkull, and yet have performed all their actions as freely as before, for a certain time, becauſe the nerves had not ſuffered any diſorder. Thus tho' the greateſt part of our ox's brain was petrified, yet the animal lived, becauſe the nerves could receive and diſtribute the ſpirits, or perhaps prepare them themſelves. We ſhall be eaſily led to embrace this opinion, if we recollect what ſeveral authors have written, that children have been born at their full time without any brain ; and M. *Mery* alſo, whoſe merit is well known, has ſhewn me the ſkeleton of a child, which had neither brain nor ſpinal-marrow, and yet had all its nerves diſtributed as uſual.

The remark, which *Bartholin* has made in ſpeaking of the obſervation already mentioned, confirms what I ſay, that the tender and ſpongy ſubſtance, which is found in ſome parts of the brain of our ox, has been able to furniſh ſome ſpirits to the nerves. For this author finding it difficult to conceive how the ox, which had its brain petrified, could live till he was knocked down by the butcher, and ſuſpecting that there muſt be in the brain ſome open *ſinus's*, thro' which the animal ſpirits paſſed freely from the arteries and nerves ; he was confirmed in this opinion by M. *Bielke*, the *Swediſh* ambaſſador, who aſſured him, that in ſeveral places of the brain there were holes, which would eaſily admit a ſtraw. After all, in what manner ſoever the ox was able to live, it muſt be acknowledged that this petrified brain is a ſort of prodigy ; as we ſee every day, that ſlight wounds made in the brain, or only in its membranes, cauſe general perturbations, which deprive it of all its functions.

An explanation of the figures.

Plate II. *Fig.* 1. AAAAA. The circumference of the brain.

BBBB. The two great lobes.

CC. The proceſſes, which riſe above the upper part of the brain.

DDD. The ſpace, which begins between theſe two eminences, and is continued below the baſe of the ſkull.

E. A blunt proceſs, which proceeds beyond that on the right ſide about a finger's breadth.

FFFF. Several windings ſeparated and half opened, which form ſinuoſities and depreſſions.

GGGG. The eminence, which I take to be the pineal gland, both by its figure and ſituation; and by its inſertion, by means of which it has preſerved a ſort of play between the *cerebrum* and *cerebellum*.

Fig. 2. repreſents the brain ſeen ſidewiſe.

HHHH. The *cerebellum*.

I. The anterior *proceſſus vermiformis*, which is raiſed above the left lobe.

L. The poſterior *proceſſus vermiformis*.

M. The cavity between the *cerebellum* and the proceſs of the upper part of the left lobe; the bottom and ſides of which ſeem leſs hard, and only oſſified.

N. The inferior proceſs of the left lobe.

O. The ſuperior proceſs.

PP. The pineal gland, ſituated in ſuch a manner, that its articulation with the *cerebrum* and *cerebellum* is eaſily ſeen.

RR. The ſuperior proceſs of the right lobe, made in form of a ſaddle.

S. A portion of the left lobe, which was cut by the butcher.

Fig. 3. repreſents the ſame brain ſeen in its upper part, where we ſee different cavities, which go to the baſe of the ſkull, marked TTT.

VIII.

VIII. *Remarks on the inequalities of the motion of pendulum clocks, by M.* de la Hire*.

The aſtronomers, who have taken great care to regulate the motion of their pendulums at ſeconds by the motion of the ſtars, have obſerved ſome inequalities in them, which they have not been able to reduce to any certain rules. I made ſome remarks on theſe inequalities, in the memoir, which was read by me to the academy, and printed in the memoirs of 1700†, and amongſt the reſt, on thoſe which may come from a little ſpring plate, which I had put inſtead of ſilk, to ſuſtain the pendulum; for I had thought, that this plate not being ſubject to the alterations, which happen to the ſilk by the dryneſs and moiſture of the air, the vibrations of the pendulum might be much more equal; but, at laſt, I was obliged to take away the plate, and put the ſilk again, becauſe I obſerved much greater inequalities than before; and I afterwards found, that the clock went pretty true, ſometimes not varying from the mean motion a ſingle ſecond in 4 days, wherein the pendulum makes 345,600 vibrations. But I have alſo ſometimes obſerved, that from one day to another there were alterations, conſiderable enough to embarraſs an exact obſerver, and to exerciſe a philoſopher, who would ſearch out the cauſe, which muſt neceſſarily be phyſical.

The different ſtates of the air ſeem to be the ſole cauſes of the alterations, which we obſerve in the motion of the pendulums: for it is hot or cold, dry or moiſt, light or heavy, thick or thin; all theſe different qualities mixing together in different degrees, may cauſe great alterations in the motion of clocks. But to find any thing that is to happen, we muſt conſider theſe different ſtates ſeparately.

---

* Dec. 15, 1703. † Vol. I. Pag. 221 of this abridgment.

We ſuppoſe, in the firſt place, that if the cycloid is well made, according to the rules which M. *Huygens* has given, all that can accelerate or retard the motion of the wheels, will not make the clock alter, ſince there could only happen longer or ſhorter vibrations, which would not fail to be iſochronical, or of equal duration. Thus cold being able in ſome meaſure to fix the little oil that is faſtened to the pivots of the wheels, will cauſe their motion to be more difficult, than in a warm ſeaſon, when the oil ſhall be more liquid, and conſequently the vibrations will become ſhorter; but yet they will be of equal duration with thoſe which are longer, being rectified by the figure of the cycloid.

The moiſture that clings to the wheels and pinions, may cauſe almoſt the ſame effect, without any inequality happening to the motion.

But tho' the cycloid is the neceſſary figure to make the longer or ſhorter vibrations be iſochronical, it ſhould be conſidered, that it could have place only when the ſuſpenſion has no bigneſs or thickneſs, which is impoſſible in the execution; wherefore, as we make uſe of a twiſted ſilk, thick enough to ſuſtain the lentil of the pendillon or pendulum, which is heavy, and as we muſt neglect nothing that may contribute to the juſtneſs of this motion, the figure muſt not be a cycloid, but a line parallel to the cycloid, which may be diſtant from it toward the concave ſide, by half the thickneſs of the ſilk, that the *axis*, or middle of this ſilk, may deſcribe the cycloid exactly, as I have explained in my *treatiſe of* *epicycloids*, which may ſerve for the motion of machines.

We may alſo obſerve, that the little threads of ſilk, which compoſe the twine, are dry and ſtiff, and that they may conſequently ſuffer altogether conſiderable alterations, almoſt like thoſe of the plate ſpring, which is ſtiffer in cold and dry weather, and ſofter in hot; but it is an accident that we cannot avoid, when we make a flexible ſuſpenſion for the pendulum; wherefore we might try what I propoſed in the above-mentioned memoir.

If we conſider the different ſtates of the air, with regard to the pendulum, and not the wheel-work of the clock, we ſhall obſerve ſo many different accidents, that we ſhould hardly be able to think a clock can go equally for an hour together, during which the pendulum makes 3600 vibrations.

We know, that the heat of the ſun in ſummer is ſtrong enough to heat a bar of iron of 6 feet long, and to make it longer than it was in winter, when expoſed to the froſt, by $\frac{2}{3}$ of a line, as I found by a very exact experiment, which I made formerly. Wherefore theſe two different ſtates of the air, with regard to the length of the pendulum, which ſhould be 3 feet, 8 lines $\frac{1}{2}$, to ſwing ſeconds, might alter $\frac{1}{3}$ of a line, which would cauſe a very conſiderable difference in the duration of the vibrations of the pendulum, ſince it might go as far as $32''$ in a day. But as this caſe could not happen, but when the clock is expoſed to the air and ſun in theſe two ſeaſons, which is not frequent, we do not often obſerve ſuch great alterations. There ſometimes happen pretty conſiderable differences of heat from one day to another, and from night to day, to make the rod of the pendulum lengthen or ſhorten, which may retard or accelerate the motion of the clock ſome ſeconds, as we alſo ſometimes obſerve, which may come from this cauſe alone. Wherefore in the uſe that is made of pendulum clocks for celeſtial obſervations, where it is neceſſary to know the hour with the utmoſt exactneſs, they muſt be placed where they are as much ſheltered as poſſible from all the injuries of the air.

The moiſture, dryneſs, denſity and rareneſs of the air, may alſo cauſe conſiderable alterations in the motion of the pendulum. For when the air is moiſt, that is, when it is filled with a quantity of ſmall particles of water, which remain ſuſpended in it, or when it is denſe or thick, the pendulum will find a difficulty in cleaving it, and its vibrations muſt then be of much longer duration, than when it is dry or rare. For we know by experience, that a very light feather falls in a tube, out of which the air has been pumped, almoſt as ſoon as a ſtone does in the air. But as we muſt not judge of what is to happen on theſe occaſions, without making the experiment if poſſible, I have thought that if the moiſt and thick air can make the durations longer, than a dry and thin air, we ſhould perceive a very great difference between the motion of the pendulum in air and in water. To know what it was, I made a pendulum of half ſeconds, with a ball of lead weighing 2 ounces, which was ſuſpended by a fine thread, and I put it in motion in the water. I obſerved, at firſt, that the great vibrations were quickly ſhortened, and that the motion ſenſibly diminiſhed after a little more than a minute. But, as I was perſuaded, that theſe vibrations in the water muſt be at leaſt each of them a ſecond, which were but $\frac{1}{2}$ a ſecond in the air, I was very much ſurpriſed to find, that they appeared to me almoſt as quick, or of equal duration, with thoſe which were made in the air. To meaſure them exactly, I cauſed the vibrations of the clock pendulum of ſeconds to be counted, during one minute, and at the ſame time I counted the vibrations of the pendulum of half ſeconds, in the water of a great broad veſſel, where the ball was immerged about half an inch, and I found, after having ſeveral times repeated the ſame experiment, that the pendulum in the water made but 112 vibrations inſtead of 120, which it ſhould have made in the air, in one minute.

I made alſo the ſame experiment with a ſingle pendulum of ſeconds, the ball of which was of lead, and weighed 5 ounces, and I found, as in the other, that the great vibrations continued but a very little while, and that the pendulum almoſt ſtopped entirely, after 2 minutes. But it made no more than 114 vibrations in the water, whilſt the pendulum of the clock made 120 in the air in two minutes. Thus the retarding cauſed by the water in the vibrations of the pendulum, is 3″ in a minute. I could have been glad to make the obſervations of theſe differences of vibrations in the water and air for 20′ or 30′, to know more exactly their difference, and to ſee what proportion there was in the retarding of the vibrations in the water, in theſe pendulums of different lengths, but I could not proceed any farther.

As a pendulum of ſeconds loſes in the water 3″ in a minute, it would loſe in a day 4320″. But if we ſuppoſe, that this diminution of the motion of the pendulums, comes from the denſity of the *medium*; and if the air is denſe by the weight with which it is loaded, without having regard to the quantity of particles of water mixed in it, it will follow, that if the weight of the air changes only $\frac{1}{28}$, as is pretty often obſerved in the barometer, $\frac{1}{28}$ of $4320''$ of retarding the pendulum in the water for one day, which is $154''$, will be the diminution or retarding of the clock in the ſpace of a day, with regard to theſe two different ſtates of the air. But we have never obſerved ſo great a difference as that in pendulum clocks; we cannot ſay therefore, that the different weights, with which the air may be loaded, can cauſe its different denſities; or elſe we muſt confeſs, that its different denſities have not the ſame effect on the motion of the pendulum, as the denſity of the water has, which may proceed from the different configuration of the parts of theſe two bodies, of which thoſe of the air, tho' very cloſe and compact, may be eaſily ſeparated; and, on the contrary, thoſe of the water not without great difficulty, being adherent to each other. We may add alſo, that the laſt vibrations in the water, being ſhorter than the firſt, go faſter.

It would be for this reaſon alſo, that the air, tho' filled with particles of water, would but little, if at all, retard the motion of the pendulum, becauſe all theſe particles not being connected, but quite ſeparated by the particles of the air, may be very eaſily diſplaced between the particles of the air, wherein they float.

But if theſe particles of water do not retard the motion of the pendulum, by the difficulty of being diſplaced, they may cauſe a conſiderable alteration by another mean. If the air, from being dry, becomes moiſt, it is certain that a very great quantity of theſe particles of water muſt ſtick to the ſurface of the rod, and of the weight of the pendulum, and they may even penetrate them a little; and they will conſequently make a ſort of covering upon the rod and lentil of the weight, which will have its centre of oſcillation different from that of the compound of the rod and weight; wherefore the centre of the oſcillation being then different from what it was before, the duration of the vibrations will not be the ſame as it was. Not that we can in any degree remedy this accident, in uſing for a pendulum a cylinder with a very ſmall baſe, which is homogeneous in its whole length, and being ſuſpended by the extremity of its *axis*, would very near have the ſame point for the centre of oſcillation of its ſurface and of its body, and conſequently whatever alteration happened to this ſurface, provided it was equal in all its parts, the motion of the pendulum would not be ſenſibly altered. It would be the ſame thing, if inſtead of a cylinder we made uſe of a parallelipipid, and provided it be alſo ſuſpended by the extremity of its *axis*.

Laſtly, if the cycloid was ill made, it might cauſe new irregularities in the motion of the pendulum, according as its vibrations ſhould be longer or ſhorter, of which ſeveral others would be formed, by their combination with the firſt.

As for the different lengths of the pendulum in different climates, it ſeems to me, that ſome rem rks might be made upon it; for M. *Picard* had obſerved, at *Uranibourg* and at *Bayonne*, where I was with him, that the length of the ſimple pendulum of ſeconds, was exactly the ſame as at *Paris*. Great attention was given to this obſervation of *Bayonne*, becauſe it was known what M. *Richer* had related at *Cayenne*. *Uranibourg* and *Bayonne* are diſtant from each other in latitude more than 12°; and between *Bayonne* and *Cayenne*, the difference of latitude is

38°,

38°, for *Cayenne* is almoſt at 5° of N. lat. which gives only a difference almoſt triple of the firſt, for which we find ¼ of a line of diminution of the length of the pendulum. We muſt therefore conclude, that this difference of length does not become very ſenſible, till we approach the line.

But ſeveral years afterwards M. *Varin*, *des Hayes*, and *de Glos*, having been ſent towards the line, to make ſome aſtronomical obſervations there, found that in the iſland *Gorée*, which is in 14° N. lat. the length of the ſimple pendulum at ſeconds, ought to be 2 lines ſhorter than in *France*. The obſervations made at *Cayenne* and *Gorée*, leave no room to doubt of their being made with great certainty and exactneſs in all the circumſtances. However, if we would have concluded this difference of the length of the pendulum for *Gorée* by that of *Cayenne*, we ſhould have ſaid that of *Gorée* ought to be only about ¼ of a line ſhorter than at *Paris*, whereas by the obſervation it is 2 entire lines. On the contrary, if we had concluded that of *Cayenne* from that of *Gorée*, we ſhould have ſaid about 3 lines, and it is found to be but ¼ of a line.

Theſe great differences cannot by any means agree with the *hypotheſes*, which M. *Mariotte* has formed in his *Traité du mouvement des eaux*, and M. *Huygens*, in his *treatiſe of light*, and we muſt ſeek for others to explain, why the length of the pendulum is the ſame in the latitudes of 55° ¼, and 43½; why it is 2 lines ſhorter in 14° ⅔; and why at 5° it is only ¼ of a line ſhorter. But might we not ſuſpect, that this different length of the pendulum is not real, but only apparent, and that it comes only from the meaſure that we have made uſe of. For it is very true, that metals, and all bodies in general, are extended conſiderably by heat, and contracted by cold. M. *Picard* ſays, that in the length of a foot, he has obſerved a prolongation of $\frac{1}{4}$ of a line; and conſequently it ſhould be $\frac{3}{4}$ of a line in the length of the pendulum, whereas I have found it but $\frac{2}{3}$ of a line. This difference might come from the different ways, in which the obſervations were made; for M. *Picard* having expoſed bodies to the froſt, put them afterwards near the fire, and for my part, I only expoſed them to the ſun the ſummer following. We might therefore ſay, that towards the line and between the tropics, where the heats are very great, metals extend and lengthen very conſiderably, beyond what they do in theſe countries; and perhaps alſo by a particular cauſe of the vapours and exhalations, which penetrate them, as we know they are very penetrating in thoſe countries; and laſtly, more at one time than another, and more in one place than another. Wherefore theſe cauſes of extenſion, which are not conſiderable in theſe countries, may be very different at *Gorée* and *Cayenne*, and at different times; for we are perſuaded, that towards the tropics, the heats are much ſtronger than towards the line. And if the iron rod of 3 feet, meaſured at *Paris*, at the time of M. *Richer*'s departure, was prolonged $\frac{5}{4}$ of a line at *Cayenne*, he muſt have found the length of the ſimple *pendulum* at ſeconds, meaſured with this rod, ſhorter than at *Paris* by $\frac{5}{4}$ of a line, tho' in reality it was the ſame in both places. In like manner, if at *Gorée* the meaſure was prolonged 2 lines more than it was at *Paris*, the length of the ſingle pendulum at ſeconds, would have appeared ſhorter than at *Paris*, by 2 lines. This ſeemed to me the moſt probable about this *phænomenon*. If it was ſo, the univerſal meaſure of the pendulum would remain always the ſame, over all the earth, and the particular meaſures ſhould be regulated by this meaſure, taking the length of the ſimple pendulum for 3 feet, or ½ a toiſe.

An examination of the demonſtration, which Meſſ. Mariotte *and* Huygens *give of the different lengths of the ſimple pendulum of ſeconds, in different parts of the earth.*

We have here, according to M. *Mariotte*, only to demonſtrate, whether bodies fall more ſlowly under the equinoctial than any where elſe; and whether they fall faſter as they approach nearer to the poles. This is what he propoſes to do in his *Traité de mouvement des eaux*, p. 245. ſuppoſing the motion of the earth about its *axis*.

He ſays, the motion of the earth gives the air an impreſſion, which makes it endeavour to fly from its *axis*, with a velocity proportionable to that of its motion; and that this motion being greater towards the equinoctial than towards the poles, the effort which it makes towards the equinoctial, is greater than that which it makes towards the poles; and it is from this different effort, that he concludes that the bodies, which are in the air, are repelled and driven from the earth with more force near the equinoctial, to hinder them from falling, than when they are near the poles.

This reaſoning of M. *Mariotte*, is founded only upon the ſuppoſition that the air, which ſurrounds the earth, is repelled from it by its motion about its *axis*; having perhaps been perſuaded of this effect by a common experiment, that if we move an irregular body in the air, the air being ſtruck by its inequalities, endeavours to fly from the body in lines perpendicular to the motion of the body; but it ſeems to me, that the ſame thing cannot happen to the globe of the earth, ſuppoſing its daily motion about its own *axis*.

For in the firſt place there is too little earth, and the inequalities of it are too ſmall, in proportion to the ſmooth ſurfaces of the waters, to part the air ſenſibly from the earth, and conſequently the ſole motion of the ſurface of the earth would make all the bodies of this ſurface ſhock the air with as great a velocity, as that of theſe bodies, which we might take for a very violent wind from E. to W. which however would have no determination to fly from the ſurface of the earth, and the particular cauſes of the winds could not have force enough to reſiſt it. If we perceived between the tropics any motion from E. to W. there are very often great calms alſo; and we might give other phyſical reaſons for this motion, beſides that of the earth; and moreover, what proportion is there between the velocity of this wind, and that of the ſurface of the earth, which travels 9000 leagues in a day.

It muſt therefore be allowed, that the atmoſphere, which encompaſſes the earth on all ſides, makes as it were but one body with it; and in the ſuppoſition of the motion of the earth about its *axis*, the atmoſphere is carried along as its ſurface. Whence it follows, that a ſtone, which ſhould fall in this atmoſphere, would not receive any impreſſion from the motion of the earth, as it would happen to a ball of lead let fall in a veſſel full of water, whilſt the veſſel ſhould be carried along by a very quick horizontal motion; for there is no doubt but this ball would fall to the bottom of the veſſel in the ſame place where it would fall, if the veſſel was at reſt, ſince the water contained in the veſſel is at reſt there, with reſpect to the maſs of water, and the ſides of the veſſel whilſt it is in motion.

And if it was poſſible for the air to be thrown off from the ſurface of the earth, by the motion of the earth, as M. *Mariotte* ſuppoſes, whether by a tangent, which would fly off from E. to W. or by a *radius* of the centre towards the circumference, the weight of the pendulum, which deſcends and riſes again in the ſame vibration, which goes on one ſide in one vibration, and on the other in the other, will always be as much accelerated in aſcending, as retarded in deſcending, and as much accelerated on one ſide, as retarded on the other; whence it follows, that no alteration can happen from this cauſe in the duration of the vibrations of the pendulum.

But in ſhort, if we ſhould allow M. *Mariotte* all that he pretends to conclude from his *hypotheſis*, it would always follow, that for the neareſt degrees to the poles, the augmentation of velocity of the motion of the pendulum would be much greater than for the degrees towards the equator; ſince this augmentation would be in the proportion of the diminution of the motion of the matter, which would be that of the ſines of the complement of the degrees of latitude, which diminiſh much faſter as we approach the poles, than towards the equator, which is contrary to the obſervation made at *Uranibourg*, and at *Bayonne*, and alſo contrary to the irregularity, which is found between *Cayenne* and *Gorée*.

As for M. *Huygens*, who did not print his *treatiſe of light*, till ſeveral years after M. *Ma-*

riotte's *Mouvement des eaux* was publiſhed, he ſays, *we cannot queſtion its being a ſign that bodies deſcend more ſlowly near the equinoctial than in* France. This is alſo what M. *Mariotte* had ſuppoſed, and for his demonſtration he adds, that *he found as ſoon as ever this new* phænomenon *was communicated to him, that the cauſe of it might be referred to the motion of the earth, which being greater in each country, according as it approached nearer to the equinoctial line, muſt produce a greater effect in throwing off bodies from the centre, and thereby taking from them a certain part of their gravity:* It is eaſy to ſee, by his own words, which I have juſt related, that he makes uſe of the ſame *hypotheſis* with M. *Mariotte*, and he afterwards determines the quantity of the diminution of this effort by his third theorem *de vi centrifuga.* Wherefore all the reaſons which I have related againſt the explanation of this *phænomenon* by M. *Mariotte*, would alſo ſerve as well as this, which concludes only the ſame thing from the ſame principle. Whence, I ſay, there muſt be ſome other cauſe of this effect, which does not depend on the motion of the earth.

As for what regards the obſervation, it ſeems very eaſy to be done, ſince we may count the vibrations of the ſimple pendulum for one hour, where it always continues in motion after it has been once put in motion, and that if the pendulum was to be two lines ſhorter, that which ſhould be two lines longer would make in an hour about 8 vibrations leſs than the other, which is a very great difference to be miſtaken in. It will be the ſame thing in the other lengths in proportion. It muſt however be obſerved, that if a thread of the *pitta*, or ſilk graſs, be made uſe of to ſuſtain the weight, let this thread be ever ſo ſlender, it is always flat, and it happens, that the laſt vibrations uſually become turning, from being ſtrait as they were at firſt, as I have tried, becauſe this thread cleaving the air obliquely in its motion, turns the pendulum one way in going, and another way in returning, which gives it by degrees a determination to turn. I have alſo obſerved, that theſe laſt turning vibrations, which ought to be ſhorter than the firſt, becauſe they are of leſs extent, are of longer duration than the ſtrait ones, which may impoſe in the obſervation. And if M. *Picard* has had regard to theſe turning vibrations, and theſe other gentlemen, who have obſerved near the line, have given no attention to it, they will have found much fewer vibrations in an hour in the ſimple pendulum, which turned a little, than there ought really to be, if the vibrations had not turned; and this will have made them judge, that the pendulum was to be ſhortened in order to make its vibrations in leſs time.

I ſhall now give an example of what I ſaid before, that there are not ſuch great alterations obſerved in the motion of the pendulum of the clock, as ought to happen from the different condenſations of the air.

Obſervations on the barometer, and on the conſtitution of the air, from *Nov.* 22, to *Nov.* 29, 1703, in the morning.

		Inches.	*Lines.*	
Nov. 22	barometer	27	$4\frac{5}{6}$	foggy weather.
23	———————	27	1	rain.
24	———————	27	$3\frac{1}{2}$	rain.
25	———————	27	$9\frac{4}{6}$	fair.
26	———————	28	$1\frac{4}{6}$	a thick fog
27	———————	27	$11\frac{1}{2}$	a thick fog.
28	———————	27	$9\frac{4}{6}$	a fog.
29	———————	27	7_{6}	fair.

I have found, by the obſervations on the ſun's paſſage thro' the meridian, and by the equation of the clock, that between the 22d and 25th, when the barometer was very low and at a mean height, and the weather partly rainy and partly fair, that the clock had gained upon the mean motion of the ſun $9''\frac{2}{3}$ in a day; and that between the 25th and 26th, when the barometer was very high, which ſhewed a very great condenſation of the air, with a very thick fog, that the clock had gained but $6''$, which would ſhew a retardation of the motion of the pendulum of $3''\frac{2}{3}$ in a day. But from the 26th to the 29th, when the barometer was always pretty high, with a great deal of fog, the clock had gained $9''$ in a day. Whence it is eaſy to ſee, that for a very great difference in the conſtitution of the air, when its weight joined with the fog ought to have greatly retarded the motion of the pendulum, as I have before obſerved, it was found however to be but a few ſeconds, which might be aſcribed to other particular cauſes.

IX. *A way to raiſe a great veſſel upon the hold, as it is conſtructed in the port of* Toulon, *without making uſe of any machines, by M.* de la Hire *.

There muſt be on each ſide of the hold a trench, in which the water is every where about ſix feet deep, and large enough to contain ſmall veſſels or barks according to their breadth; and theſe veſſels, if ever ſo much loaded, muſt draw no more water than the ſix feet in the trench.

---

* Dec. 22. 1703.

The great veſſel, which we would raiſe upon the hold, having been drawn againſt the hold, we muſt place on each ſide 2, 4, or 6 of the little veſſels, as many as ſhall be neceſſary for the following operation, and fill them with as much water as they will bear without ſinking.

Then place great maſts acroſs the breadth of the great veſſel, paſſing beyond both ſides, to be ſuſtained upon treſſels placed, and reſted upon the deck of the ſmall veſſels †. The maſts muſt be well faſtened to the body of the great veſſel, either with chains or cables, embracing it underneath, and faſtened to the maſts or otherwiſe.

Which being done, begin to empty all the water out of the ſmall veſſels, which will proportionably riſe towards the ſurface of the water; and raiſing the maſts with them, will raiſe the body of the great veſſel as high as they riſe themſelves.

In this ſtate, the great veſſel may be brought forwards with the ſmall veſſels, without any difficulty, till it comes to the hold, upon which it will be raiſed according to the elevation of the ſmall veſſels, and reſted upon the hold in that place, in ſuch a manner, that it cannot fall or ſlip towards the foot of the hold, when the ſmall veſſels ſuſtain it no longer.

Which being done, fill the ſmall veſſels with water, as at firſt, and place other treſſels than the firſt, being high enough to touch the maſts, which croſs the great veſſel.

Now, if you empty the water out of the ſmall veſſels, they will riſe, and alſo raiſe the maſts as they did at firſt; and conſequently they will alſo raiſe the body of the great veſſel, to which the maſts are faſtened, in ſuch a manner, that the great veſſel will no longer touch the hold at the place where it was, and it ſhall be drawn againſt the hold, as it was at firſt; but it will be much more elevated. It will be kept alſo in this place by means of the great cables faſtened on the top of the hold.

---

† Plate II. Fig. 4.

By this means, repeating the operation as often as ſhall be neceſſary, we may make the veſſel riſe above the hold, and draw it entirely out of the water, provided the quantity of water, with which the ſmall veſſels are filled, is equal at leaſt in bulk to that which the great veſſel occupies at firſt, before it begins to riſe upon the hold. And as we may eaſily meaſure the ſpace or bulk, which the great veſſel occupies in the water, we ſhall have by this means the magnitude and number of the ſmall veſſels, to be uſed in raiſing the great veſſel.

When I ſaid the trenches on both ſides of the hold ſhould be ſix feet deep in water, it was only to eſtabliſh a mean meaſure; for if the water is deeper, we may uſe larger barks to raiſe the veſſel; and if it was not poſſible to make them of this depth, they muſt be broader, and in greater number, that is, as many as the length of the veſſel would permit, being ranged on one ſide, according to their length.

It muſt be obſerved, that, when the veſſel has begun to riſe upon the hold, the fore-part will be more elevated than the back-part; wherefore the ſmall veſſels, which ſhall be placed on the ſide towards the fore-part, muſt have higher treſſels to ſupport the maſts in that place, than thoſe which are towards the hinder part, that the whole body of the veſſel may riſe in the ſame inclination, in which it was placed on the hold.

It

It will be alſo very neceſſary to put a cradle under the veſſel, to keep it upon the hold, without leaning to one ſide or the other, and to defend it from accidents, which may happen to it from its own weight.

X. *A deſcription of the heart of a ſea tortoiſe, by M.* Mery *.

The vital parts of this tortoiſe were contained together with the natural parts in the ſame cavity. The lungs occupied the upper part of it entirely. They were faſtened to the back, from the neck quite to the tail; the heart was placed in the forepart, and the natural parts behind. There was no diaphragm to make a partition between them.

The heart of this tortoiſe was however incloſed in a *pericardium*, to the bottom of which it was faſtened by three little fleſhy ligaments. This *pericardium* was full of a liquor as clear and tranſparent as the pureſt water, in which the heart of this animal was bathed. Its figure was conical, it was 2 inches long, and 6 lines broad or thereabouts. On the inſide it was divided into 3 ventricles, one was placed to the right, the other to the left, and the third in the middle, under the right ventricle.

The left ventricle was ſeparated from the right by a fleſhy partition, which had towards the *baſis* of the heart an oval aperture, pretty like that which is found in the partition that divides the auricles of the heart of the human *fœtus*. This partition was alſo pierced by a great number of little holes, by which, as well as by the oval aperture, theſe 2 ventricles communicate together.

---

* *Extracted from the regiſters of the royal academy of ſciences for* March 12, 1704.

There were upon this oval aperture two depreſſed valves; but as in this ſtate they did not ſhut entirely, they could only in part hinder the blood from paſſing out of one of theſe ventricles into the other by this hole.

The right ventricle communicated alſo with the middle one by another aperture. This was 5 or 6 lines long to 3 or 4 broad, in the middle of its length. In this paſſage from one to the other, there was no valve; and as this ſecond hole of communication was in a manner as long as the ventricle of the middle one was deep, we can conſider this only as a continuation of the right ventricle, from which it was diſtinguiſhed only by a little contraction. The fibres, of which theſe 3 ventricles were compoſed on the inſide, not being cloſely locked againſt each other, formed a ſort of fleſhy ſponge in their capacity. The left ventricle was equal to that of the middle one; but the right ventricle ſeemed alone as big as the other two taken together.

Three trunks of arteries proceeded from the *baſis* of the heart of this tortoiſe. Two of theſe arteries had their outlets into the right ventricle, and the third into the middle ventricle. Theſe 3 veſſels had each of them no more than 2 ſigmoidal valves at their apertures.

The 2 trunks of arteries, which parted from the right ventricle, had their diameters almoſt equal; they were both compoſed of 2 plans of fleſhy fibres, very viſible, and lying one upon another. The fibres of the outer plan were diſpoſed according to the length of theſe 2 arteries, thoſe of the inner plan ſeemed to be circular.

Theſe 2 plans of fibres were not ſenſible in the third trunk of an artery, which derived its origin from the middle ventricle; but the capacity of this alone was almoſt as great as that of the two others taken together, and its membranes alſo were leſs thick.

Of the 2 trunks of arteries, which proceeded from the right ventricle placed one by the ſide of another, the right advancing forwards divided immediately into 2 great branches. The firſt going in a right line towards the neck divided into 2 others, and each of theſe into 2 branches, 2 of which were extended into the anterior fins; theſe made the axillaries; the 2 others placed between the firſt went to the head, and formed the carotids.

The ſecond branch, bending on the right-ſide, paſſed under the right branch of the *trachea*, after which it ſlid between the lungs, to gain the hinder part of the body. Taking this courſe, it gave branches to the kidnies, the bladder, the parts of generation, and the poſterior fins. By this diſtribution of the arteries, it was very eaſy for me to judge, that this firſt trunk was that of the *aorta*, tho' it parted from the right ventricle. Its capacity was a little bigger than that of the artery, which I am going to deſcribe.

The left trunk formed on its own ſide the ſame bending which was made on the right by the poſterior branch of the *aorta*, and followed the ſame courſe. This trunk did not ſend any branch into the anterior parts of the body of this tortoiſe. It divided only beyond the liver into 3 branches, of which the firſt ſupplied the place of the cœliac, the ſecond of the meſenteric, and the third paſſing from left to right, reunited with the poſterior branch of the *aorta*, as the arterial canal of communication does in the human *fœtus.* And it is for this reaſon, that I have called this ſecond artery the canal of communication, to diſtinguiſh it from the trunk of the *aorta.*

The third trunk, which drew its origin from the middle ventricle, made the body of the pulmonary artery. This trunk divided into 2 conſiderable branches, which formed to right and left bendings like thoſe of the poſterior branch of the *aorta*, and of the canal of communication. Both of them paſſed under the branches of the *aſpera arteria*, to go one to the right, and the other to the left lobe of the lungs. The circuit of this third trunk was almoſt equal to that of the *aorta*, and of the canal of communication taken together. Theſe 3 arteries were joined together by their exterior membranes, from the heart, to their diviſion into branches.

All theſe roots of the veins of each lobe of the lungs uniting together, formed at their coming out of the lungs a vein on each ſide, the capacity of which was half as big as that of the 2 pulmonary arteries, which deſerves attention. Theſe 2 veins went to the left auricle, at the outlet of which they joined together by their extremity, without forming after their union a canal, which had alone the capacity of theſe 2 veins taken together; thus they did not form any trunk, each of them poured the blood which it brought, immediately into the capacity of this auricle.

The veins, which brought back to the heart the blood of all the other parts of the body, did the ſame; ſo that there was no ſingle trunk, to which alone we can truly give the name of *vena cava.* For tho' in appearance they all formed by their union a crooked canal, joined to the auricles by its convex part, to the liver by its concave part, the middle of which anſwered to the aperture of the right auricle; yet this canal, in this place, ſeemed a little narrower than in its lateral parts. Theſe 2 parts therefore made 2 diſtinct trunks, ſeeing in the place of their union they did not form a duct, which had alone the capacity of both joined together. The blood of both of them flowed immediately into the right auricle. The axillary veins, which opened into theſe 2 trunks, were filled with fleſhy fibres, which formed, by their interlacing, a ſort of tuft, of a wonderful ſtructure, of which there were ſome rudiments in the conflux of the 2 *venæ cavæ.*

The right auricle had at its outlet 2 valves, which formed together an oval aperture 7 or 8 lines long, and 3 or 4 broad in the middle. This aperture made the communication of the veins juſt mentioned with this auricle.

Theſe 2 valves are ſo much more worthy of obſervation, as there was none at the inoſculation of the left auricle with the pulmonary veins.

Of the 2 auricles of the heart the right was the biggeſt, its capacity ſeemed double of that of the left auricle, which is obſervable, as well as the difference between the capacity of the pulmonary veins and arteries to determine pretty nearly the quantity of blood which paſſes thro' theſe veſſels, and its different velocity.

Theſe 2 auricles were filled with fleſhy fibres, which being connected together different ways, formed a ſort of little net, and alſo little cells, which were pretty deep. Theſe auricles were joined together on the outſide; but on the inſide they were ſeparated by a partition, which was but ½ a line in thickneſs. This partition was partly fleſhy, and partly membranous.

Its membranous part, made in form of a half moon, falling perpendicularly upon the *baſis* of the heart, united with it, and divided it into two; ſo that it divided the outlet of the right ventricle from that of the left.

At the membranous part of this partition, two ſemilunar valves were faſtened and ſuſpended. Theſe valves being depreſſed, one into the right ventricle, and the other into the left, partly cloſed the *foramen ovale*, which made the communication of theſe two ventricles. In this ſtate, theſe valves formed one cavity between them: being elevated they deſcribed a plane parallel to the *baſis* of the heart, but in this ſituation they could ſtop no more than half the openings of the auricles into the ventricles, becauſe of their being made in form of a half moon.

Of theſe two valves, that which poſſeſſed the entrance of the right ventricle had no companion, but that which was placed at the outlet of the left ventricle, was accompanied by two others much ſmaller than itſelf; theſe had not the liberty of riſing, which the other had, becauſe they were faſtened to fleſhy columns, which bound them within to the ſide of the left ventricle.

XI. *A deſcription of the heart of a great* American *land tortoiſe, with reflections upon that of M.* du Verney *, *by the ſame.*

The figure A †, which repreſents the heart of the *American* land-tortoiſe, reverſed in the fore part, with the auricles and veins in their natural ſituation, reſembles a kidney a little flatted at the top and bottom; ſo that it is much more broad than long. Its baſis ‡ is a little concave, and is naturally turned towards the head of this animal.

---

* See pag. 137 of this abridgment. † Plate III. Fig. 1.

The vital and the natural parts are incloſed in the ſame cavity, becauſe there is no diaphragm to part them. M. *du Verney* ſeems to me therefore to be miſtaken, when he ſays, that as the tortoiſe walks on its 4 feet, *the heart is ſituated in the upper part of the* thorax *above the liver.* It is naturally placed in the fore part of this cavity, which contains all theſe parts together; which does not hinder the heart from being alone, and particularly incloſed in a *pericardium*.

I have obſerved 7 veins near the heart ‖; the two *cavæ* BB, the two axillaries CC, the coronary D, and two other veins EE, which I call hepatic, becauſe they derive their origin only from the liver. The four firſt are very conſiderable; the other three are much leſs ſo.

The two *venæ cavæ* proceed from the hinder parts of the tortoiſe, from which they bring back the blood to the heart. Paſſing by the liver, one to the right and the other to the left, they receive a great number of roots of veins from that bowel.

Theſe two veſſels do not form any particular trunk in the place of their concourſe. There, on the contrary, they ſeemed to have a little leſs capacity than before their union, tho' the coronary vein and left hepatic come hither. The right axillary and hepatic open into the right *vena cava* an inch from the right auricle: but the left axillary joins itſelf only on the ſide of the left *vena cava*, very near the entrance of this auricle. They are the two axillaries, that the jugulars are joined to, which bring back to the heart the blood of all the anterior parts; all theſe veins appear to

‡ Fig. 2.

‖ Fig. 1.

to be merely membranous, their inner ſurface is as ſmooth as the outer. From the known ſtructure of theſe veins, I draw two conſequences againſt M. *du Verney*'s deſcription.

The firſt is, that he has not been able to ſee *about the heart of theſe animals, a ſort of reſervoir of an oblong figure, and pretty like that of a blown bladder, formed by the concourſe of ſeveral veins.* The ſecond conſequence is, that it is falſe, that this pretended reſervoir is *lined on the inſide with fleſhy fibres, which croſs one another, and interlace almoſt like thoſe within the auricles of the human heart.* Nor is it true, that *the* vena cava *is lined in like manner thro' about an inch of its length, and the outlets of the other veſſels.*

Fig. 1. which repreſents the ſeven veins juſt deſcribed, is a deciſive proof, 1. That the great reſervoir, which M. *du Verney* ſays is formed of the aſſemblage of theſe veins, does not exiſt in the land tortoiſe of *America.* 2. That the texture of fleſhy fibres * exiſting only in the axillary veins of the ſea tortoiſe, it is evident, that the obſertion made by this ſkilful anatomiſt, is certainly taken from the obſervations which I made on theſe veins in 1685.

The two pulmonary veins † FF do not form any more than the two *cavæ*, a particular trunk by joining together: on the contrary, the place of their union ſeems more contracted than any other part. It is therefore falſe, 1. That *they go to form a ſecond reſervoir, much ſmaller than the firſt*, as M. *du Verney* ſhews in his figures. And becauſe the inner ſurface of theſe two veins is no leſs ſmooth than that of the *venæ cavæ*, it is alſo falſe, 2. That *the baſon* of his *little reſervoir* is *alſo furniſhed on the inſide with fleſhy fibres.* The two reſervoirs, which this ſkilful anatomiſt repreſents to us in 8 figures, and the texture of fleſhy fibres, which he aſcribes to them, are therefore at laſt quite imaginary.

---

* Fig. 9. † Fig. 8.

The two auricles GG repreſent on their outſide only one blind ſack, lying tranſverſly on the *baſis* of the heart; but this ſack is divided on the inſide by a partition I ‡, into two cavities of different bigneſs. This partition is fleſhy both in its upper and lower part.

The capacity of the left auricle * HH is ſmaller by half than that of the right auricle KK †. In both of them there are a great number of fleſhy fibres.

In the angle formed by the left auricle with the partition I, which ſeparates it from the right, there is an aperture L *, by which the pulmonary veins diſcharge their blood into the capacity of the left auricle. This aperture being narrower within than without, is one of the cauſes that hinder the blood of theſe two veins from returning whence it came.

But becauſe the outlet of the two *venæ cavæ* is not alſo placed in the angle formed by the ſame partition with the right auricle, which this angle might contract; nature has given this auricle two valves MM †, which leaving only a ſmall ſlit between them produce the ſame effect, that is, they oppoſe the return of the blood from the right auricle into the *venæ cavæ*.

M. *du Verney* places theſe two valves at the outlet of his great reſervoir; but ſince it does not exiſt, it is viſible, that they belong to the right auricle, of which they occupy the entrance.

‡ Fig. 3 and 5. * Fig. 3. † Fig. 5.

I have obſerved in the *American* land tortoiſe, which I diſſected, 4 ventricles, which communicate with each other by 3 ſtraits, which make the ſeparation of them. To make a juſt deſcription of both of them, I ſhall regulate myſelf according to the courſe of the blood, which traverſes them.

The firſt ventricle P *, which I call the left ventricle, both on account of its ſituation, and becauſe it receives the blood from the left auricle, communicates with the ſecond *q* †, by the firſt ſtrait R, which I call the *foramen ovale*; becauſe it pretty much reſembles that of the human *fœtus* placed in the partition, which ſeparates the auricles of the heart one from the other, and becauſe it has the ſame uſe.

The outlet of the left ventricle is furniſhed with 3 ſigmoidal valves NNN. That of the right ventricle has but one O. Theſe valves perform the office of the triglochine valves of the human heart. M. *du Verney* however tells us, there is but one valve at the entrance of the left ventricle; he is therefore miſtaken in both.

The ſecond ventricle Q, which I call the right ventricle, becauſe it is ſituated on the right ſide, and receives the blood of the right auricle, communicates with the third S ‖, by the ſecond ſtrait T. Above this paſſage, in the right ventricle, there is a fleſhy valve V, made in form of a half moon. This valve can ſhut but a ſmall part of this ſtrait; it has been hitherto unknown to M. *du Verney*. The valve O, depreſſed in the *foramen ovale*, which in this ſtate permits the blood of the right auricle to enter into the right ventricle, has not eſcaped his exactneſs; but however, 3 out of theſe 5 valves have not come to the knowledge of this attentive anatomiſt.

---

* Fig. 3. † Fig. 5. ‖ Fig. 6.

The third ventricle S *, communicates with the fourth X **, by the third ſtrait Y.

Theſe 4 ventricles communicating together, and the left and right having no arteries to bring back the blood which they receive from the auricles, it is eaſy to ſee, that the blood of the pulmonary veins, muſt neceſſarily paſs from the left ventricle into the right, and mixing with the blood of the *venæ cavæ*, muſt enter together into the third and fourth, to take the courſe of the arteries which part from theſe 2 ventricles.

According to M. *du Verney*, there are but 3 cavities in the heart of the land tortoiſe of *America*; but if we examine the heart of this animal itſelf, we ſhall find 4, without reckoning the auricles. This excellent anatomiſt is no leſs miſtaken in the number of the ventricles, than in that of the valves, with regard to which, I ſhall make the following reflection, before I proceed to the arteries.

Of the 3 valves NNN *†, which are placed at the entrance of the left ventricle P, the middle one, and the valve O *‖, ſituated at the entrance of the right ventricle Q, can but partly cloſe the *foramen ovale* R, by ſinking on each ſide, and by riſing, partly ſtop the paſſages of the auricles into theſe two ventricles in the land as well as in the ſea tortoiſe.

M. *du Verney* pretends, however, that when theſe 2 valves are raiſed, they entirely cloſe the inoſculations of the auricles with the ventricles, and that when they fall, they do not at all oppoſe the paſſage of the blood from the left ventricle, thro' the *foramen ovale* into the right ventricle, but neither of theſe obſervations agree with experience. Theſe 2 valves are faſtened to the membranous part of the partition of the auricles, which falling upon the *baſis* of the heart, ſeparates the outlet of the left ventricle from that of the right.

---

* Fig. 6. ** Fig. 7. *† Fig. 3.
*‖ Fig. 5.

From the baſis of the heart A *, proceed 3 trunks of arteries; the *aorta* 1, the canal of communication 2, and the pulmonary artery 3. The *aorta* and canal of communication derive their origin from the third ventricle S †. The pulmonary artery 3 is derived from the fourth ventricle X ‖. Theſe 3 arteries have each of them but 2 ſigmoidal valves at their outlets.

M. *du Verney* and I agree, that *three conſiderable arteries proceed from the baſis of the heart.* But as to the origin of theſe 3 arteries, his obſervations are very different from mine, for he ſays, *two of theſe arteries open into the firſt cavity of the heart*, which by his own obſervations, makes the right ventricle, as it *receives the blood from the right auricle.* Now I find, that there goes no artery from this cavity, and that the 2 arteries, which according to him compoſe the *aorta*, one of which, according to me, makes the canal of communication, far from *opening into the firſt cavity of the heart*, as he pretends, proceed from the third ventricle; and that the *third artery, which is that of the lungs, proceeds immediately* from the fourth ventricle, and not *from the third cavity of the heart*, as he pretends. This miſtake comes only from his

* Fig. 2. † Fig. 6. ‖ Fig. 7.

not having perceived, in the land tortoiſe of *America*, the 4 ventricles which I have demonſtrated to the academy in the heart of this animal.

The trunk of the *† aorta, at about the diſtance of an inch from the heart, divides itſelf into 2 conſiderable branches; one turns backwards, the other forwards. The poſterior branch 4, bending from left to right, croſſes the anterior branch 5, after which it continues its courſe towards the tail, and gives branches to all the poſterior parts of the body.

The anterior branch 5 advancing towards the head divides into two branches, each of which ſubdivides into two others, which make the axillary arteries 8, 8, and the carotids 9, 9, which throw themſelves into all the anterior parts.

The ſecond trunk of an artery * 2, 2, 2, 2, which I call the canal of communication, becauſe it diſcharges a part of the blood which it receives from the heart into the poſterior branch 4 of the *aorta*, bending alſo backwards, but on the left ſide, at firſt croſſes this branch, and after having produced the cœliac artery 6, and the meſenteric 7, it unites with it, and opens into its capacity.

The pulmonary artery † 3, which makes the third trunk, repreſented open, is divided into two conſiderable branches, which croſs one another from their origin; ſo that the right paſſes into the left lobe, and the left branch into the right lobe of the lungs.

The trunks of theſe three arteries are of unequal capacity. That of the canal of communication 2, is a little ſmaller than that of the *aorta* ‡ 1, but that of the pulmonary artery ** 3, is

---

*† Fig. 2 & 4. * Fig, 4. † Fig. 7. ‡ Fig. 6.

itſelf almoſt as big as that of the *aorta*, and that of the canal of communication taken together, in both tortoiſes.

In the land tortoiſe, the pulmonary arteries have a capacity equal to that of the pulmonary veins. In the ſea tortoiſe the pulmonary veins have a much leſs capacity than the pulmonary arteries.

An explanation of the figures in plate III.

Fig. 1. A. The heart reverſed.
B.B. The *venæ cavæ.*
C. C. The axillaries.
D. The coronary.
E. E. The 2 hepaticks.
F. F. The 2 pulmonary veins.
G. G. The 2 auricles.
Fig. 2. A. The heart in its natural ſituation.
G. G. The auricles.
1. The *aorta.*
2. 2. The canal of communication.
3. 3. 3. The pulmonary artery, and its two branches.
6. The cœliac.
7. The meſenteric.
4. The poſterior branch of the *aorta.*
5. The anterior branch.
8. 8. The axillaries.
9. 9. The carotids.
Fig. 3. A. A. The heart.
H. H. The left auricle open.
L. The aperture of the pulmonary veins.
N N N. The 3 valves of the left ventricle.
P. The left ventricle open.
R. The firſt ſtrait, or *foramen ovale.*
6. The cœliac.

7. The meſenteric.

I. The partition of the auricles.

Fig. 4. 1. The trunk of the *aorta.*

2. 2. 2. 2. The canal of communication.

4. 4. The poſterior branch of the *aorta.*

5. The anterior branch.

6. The cœliac.

7. The meſenteric.

8. 8. The axillaries.

9. 9. The carotids.

Fig. 5. A. A. The heart.

I. The partition of the auricles.

K. K. The right auricle opened.

M. M. The 2 valves of the inoſculation of the veins with the right auricle.

O. The valve of the entrance of the right ventricle.

Q. The right ventricle opened.

R. The firſt ſtrait, or *foramen ovale.*

T. The ſecond ſtrait.

V. The fleſhy valve.

Fig. 6. A. A. The heart.

S. The third ventricle opened.

T. The ſecond ſtrait.

1. The *aorta* opened.

2. 2. The canal of communication opened.

4. The entrance of the poſterior branch of the *aorta.*

5. The anterior branch.

6. The cœliac.

7. The meſenteric.

Y. The third ſtrait.

Fig. 7. A. A. The heart.

G. G. The auricles.

X. The fourth ventricle opened.

Y. The third ſtrait.

3. 3. 3. The pulmonary artery with its two branches.

Fig. 8. D. Part of the coronary vein.

E. The outlet of the veins of the right auricle.

F. F. The two pulmonary veins.

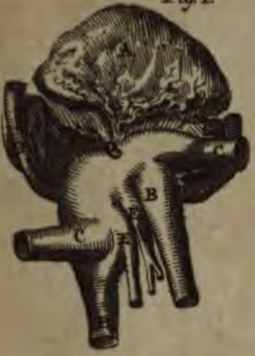
G. G. The auricles.

Fig. 9. repreſents the texture of the fleſhy fibres of the axillary veins of the ſea tortoiſe.

Pl. III. *Vol. II.*

134.

Fig. 1.



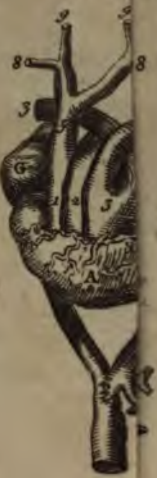


Fig. 5.

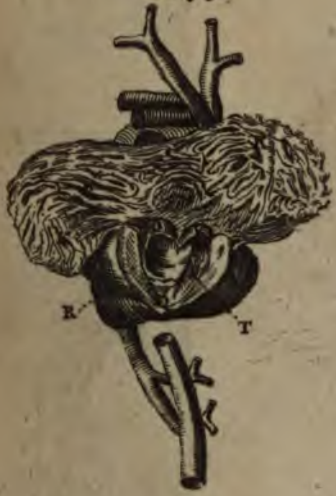


Fig. 6.

nde sc.

A

TABLE

OF THE

PAPERS contained in the ABRIDGMENT
of the HISTORY and MEMOIRS of the
ROYAL ACADEMY of SCIENCES at
PARIS, for the Year MDCCIV.

In the HISTORY.

I. OF *the earthquakes in* Italy.
II. *A diſcovery of a ſingular property in the thermometer.*
III. *Of the cauſe of the heat of lions.*
IV. *On the figure of the* extrados *of a circular vault, of which all the arch-ſtones are* in equilibrio *with one another.*

In the MEMOIRS.

I. *An obſervation on the quantity of rain, which fell at the obſervatory, with the heights of the thermometer and barometer, in the year* 1703, *by M.* de la Hire.
II. *New obſervations on the inſects of orange-trees, by M.* de la Hire.
III. *That all the barometers, double as well as ſimple, which have been hitherto conſtructed, act not only by the greater or leſs weight of the air, but alſo by its greater or leſs heat; and the means of preventing this defect for the future, in the conſtruction of double barometers, and of correcting the error in the uſe of the ſimple barometers, by M.* Amontons.

IV. *The hiſtory of the* Formica-leo, *by M.* Poupart.
V. *A diſcourſe on the barometers, by M.* Amontons.
VI. *Extract of a letter from M.* Sarraſin, *phyſician to the king, in* Canada, *concerning the anatomy of the caſtor, read to the academy by M.* Pitton Tournefort.
VII. *The deſcription and uſe of a level of a new conſtruction, by* M. de la Hire.

AN
ABRIDGMENT
OF THE
PHILOSOPHICAL DISCOVERIES and OBSERVATIONS in the HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1704.

I. *Of the earthquakes in* Italy.

M. *MARALDI* having communicated to the academy ſome accounts which he had received of earthquakes in *Italy*, we will here collect from them the greateſt part of the natural philoſophy which they contained.

The earthquakes in *Italy* began *Octob.* 1702, and continued till *July* 1703. The countries which ſuffered moſt by them, and where they began, are the city of *Norcia*, with thoſe depending on it in the *eccleſiaſtical ſtate*, and the province of *Abruzzo*. Theſe countries are contiguous and ſituated at the foot of the *Apennine*, on the ſouth-ſide.

The earthquakes have often been accompanied with terrible noiſes in the air, and theſe noiſes have been alſo frequently heard without any earthquake, even when the ſky has been very ſerene. The earthquake on *Feb.* 2, 1703, which was the moſt violent of them all, was accompanied, at leaſt at *Rome*, with a great ſerenity of the ſky, and a great calm in the air. At *Rome*, it laſted half a minute; and at *Aquila*, the capital of *Abruzzo*, 3 hours. It overthrew the whole city of *Aquila*, buried under its ruins 5000 perſons, and made great havock in the adjacent parts.

The ſhakings of the earth have commonly been from N. to S. or very near it, which has been obſerved by the motion of the lamps in the churches.

It made two openings in a field, from whence, with violence, iſſued out great quantities of ſtones, which, having entirely covered it, made it barren. After the ſtones, there ſprang up ſuddenly from their openings two ſpouts of water, whoſe height much exceeded the trees of this country, which laſted a quarter of an hour, and overflowed the neighbouring countries. This water is whitiſh, like ſoap-ſuds, and has no taſte.

A mountain which is near *Sigillo*, a town 22 miles diſtant from *Aquila*, had a pretty large plain on its ſummit, ſurrounded with rocks, which were a kind of wall to it. Since the earthquake of *Feb.* 2, inſtead of this plain, there is a gulph made of an unequal breadth, the greateſt diameter of which is 25 toiſes, and the leaſt 20. The bottom of it could not be found, tho' they went 300 toiſes deep. At the time when this opening was made, flames were ſeen to proceed out of it, and afterwards a very great ſmoak, which laſted 3 days with ſome interruptions.

At *Genoa*, the 1ſt and 2d of *July*, 1703, there were two ſmall earthquakes. The laſt was felt only by perſons who were at work on the mole. At which time the ſea in the port fell 6 feet; ſo that the gallies in the *Darſe* touched the bottom, and the ſea continued thus low near a quarter of an hour.

The ſulphurous water, which is in the road from *Rome* to *Tivoli*, diminiſhed 2 feet ½ in height, both in the baſon and in the ditch. In many places of the plain, called *Il Teſtino*, there were ſprings and rivulets of water, which formed im-

impaſſable marſhes, but all dried up. The water of a lake, called *Inferno*, diminiſhed alſo 3 feet in height. Inſtead of the old ſprings, which ceaſed, there came new ones about a league diſtant from the firſt; ſo that in all likelihood thoſe very waters have changed their courſe.

II. *A diſcovery of a ſingular property in the thermometer.*

It muſt appear ſomething ſurpriſing, that when we cover the ball of the thermometer with our hand to warm the liquor, and make it aſcend to the tube, this liquor begins to fall, and does not aſcend above its firſt level till after this ſeeming irregular motion, which is ſo contrary to what might have been expected. M. *Amontons*, who ſpoke of this, on the occaſion of his new thermometers, refers this motion, whereby the liquor falls at firſt, to the rarefaction which the heat of the hand cauſes even in the ſubſtance of the glaſs of the ball, before it can cauſe any in the liquor. The capacity of the ball therefore is augmented, and conſequently the liquor in the tube ſinks, till it has received warmth enough to riſe, notwithſtanding the increaſe of the capacity of the ball.

M. *Amontons* hath calculated upon exact experiments, how much the increaſe of this capacity may be, and he has found it to be but 1000th part. This 1000th part, by which the ball is augmented, and which is the quantity of the liquor which enters into it, or which ſinks, will become ſo much the more viſible on the tube, the narrower the capacity of the tube is when compared with that of the ball.

III. *Of the cauſe of the heat of lions.*

In the lion, the gall-bladder has ſeveral plaits or folds, and from thence M. *du Verney* has conjectured, that the bile being able to remain there a longer time, and exalt itſelf more, may perhaps be the cauſe of the great heat of this animal, and of the continual fever which is aſcribed to it.

IV. *On the figure of the* extrados *of a circular vault, of which all the arch-ſtones are* in equilibrio *with one another.*

A vault, or ſemi-circular arch, being placed upon its two impoſts, and all the arch-ſtones or *vouſſoirs* that compoſe this arch being cut and placed in ſuch a manner, that their joints being prolonged meet all in the centre of the arch, it is evident, that all the arch-ſtones muſt be in form of wedges, wider at the top than at the bottom, by virtue of which they bear upon and ſuſtain each other, and mutually reſiſt the effort of their weight which determines them to fall. The arch-ſtone in the middle of the arch, which is perpendicular to the horizon, and is called the key of the vault, is ſuſtained on each ſide by the two contiguous arch-ſtones, exactly as by two inclined planes, and conſequently the effort that it makes to fall, is not equal to its weight, but is a certain part of it which is greater, as the inclined planes that ſuſtain it are leſs inclined; ſo that if they were infinitely little inclined, that is, perpendicular to the horizon, as well as the key of the vault, it would endeavour to fall with its whole weight, would not be any more ſupported, and would actually fall, if the cement, which we do not here conſider, did not hinder it. The ſecond arch-ſtone, which is to the right or left of the key of the vault, is ſuſtained by a third arch-ſtone, which by virtue of the figure of the vault, is neceſſarily more inclined to the ſecond, than the ſecond is to the firſt; and conſequently the ſecond arch-ſtone in the effort that it makes to fall, exerciſes a leſs part of its weight than the firſt. By the ſame reaſon, all the arch-ſtones, reckoning from the key of the vault, proceed always in exerciſing a leſs part of their total weight; and, in fine, the laſt, which is placed upon a ſurface horizontal to the impoſt, does not exerciſe any of its weight, or, which is the ſame thing, does not make any effort to fall, as it is entirely ſupported by the impoſt.

If we would have all the arch-ſtones make an equal effort to fall, or be *in equilibrio*, it is viſible that each, from the key of the vault to the impoſt, exerciſing always a leſs part of its whole weight; as for example, the firſt exerciſing $\frac{1}{2}$, the ſecond $\frac{1}{3}$, the third $\frac{1}{4}$, *&c.* there is no other way to make theſe different parts equal, than by proportionably increaſing the wholes, of which they are parts; that is, the ſecond arch-ſtone muſt be heavier than the firſt, the third than the ſecond, and ſo on to the laſt, which muſt be infinitely heavy, becauſe it makes no effort to fall; and a null part of its weight cannot be equal to the finite efforts of the other arch-ſtones, unleſs this weight is infinitely great. To apprehend this idea in a more ſenſible manner, and leſs metaphyſically, we need only reflect, that all the arch-ſtones, except the laſt, cannot let any other arch-ſtone whatever fall without raiſing themſelves; that they reſiſt this elevation to a certain point determined by the greatneſs of their weight, and by the part of it that they exerciſe, that only the laſt arch-ſtone can let any other fall, without riſing in ſome manner, and only by ſlipping horizontally; that the weights, ſo far as they are finite, bring no reſiſtance to the horizontal motion, and that they do not begin to bring any that is finite, till we conceive them to be infinite.

M. *de la Hire*, in his *Treatiſe of Mechanicks*, printed in 1695, has demonſtrated what is the proportion, according to which the weight of the *vouſſoir* of a ſemi-circular arch muſt be encreaſed, that they may all be *in equilibrio*, which is the ſureſt diſpoſition that can be given to a vault, to render it durable. Before this, the architects had not any exact rule, and were only guided by trying. If we reckon the degrees of a quadrant of the circle from the middle of the key of the vault to one impoſt, the extremity of each *vouſſoir* will belong to an arch as much greater, as it ſhall be farther diſtant from the key; and according to M. *de la Hire*'s rule, we muſt encreaſe the weight of an arch-ſtone above that of the key, as much as the tangent of the arch of this *vouſſoir* exceeds the tangent of the arch of half the key. The tangent of the laſt *vouſſoir* neceſſarily becomes infinite, and conſequently its weight alſo; but as infinity has no place in practice, it is reduced to charge the laſt arch-ſtones as much as poſſible, that they may reſiſt the effort that the vault makes to ſeparate them, which is called the *ſhoot*.

M. *Parent* has inquired what ſhould be the outſide bend or *extrados* of a vault, of which the *intrados* ſhould be circular, and all the arch-ſtones *in equilibrio*, by their weight, according to M. *de la Hire*'s rule; for it is plain, that all theſe arch-ſtones, unequal in a certain proportion, make on the outſide a certain regular curve. He found it only by points, but in a very ſimple manner; ſo that by his method we might eaſily enough build a vault, of which we ſhall be ſure that all the arch-ſtones will be *in equilibrio.*

A conſiderable effect of the inquiries of M. *Parent* is, that he has at the ſame time diſcovered the meaſure of the ſhoot of the vault, or what proportion this ſhoot has to the weight of the entire vault. We only knew that this effort was very great, and oppoſed great maſſes of ſtones, or butments, rather too ſtrong, than too weak, but we did not know exactly what *medium* to obſerve.

AN
ABRIDGMENT
OF THE
PHILOSOPHICAL MEMOIRS of the ROYAL ACADEMY of SCIENCES, at *Paris*, for the Year 1704.

I. *An obſervation of the quantity of rain which fell at the obſervatory, with the heights of the thermometer and barometer, in the year* 1703, *by M.* de la Hire *.

WHEN I undertook to make exact obſervations on the quantity of rain-water which fell at the obſervatory in the courſe of a year, I had no other view than to draw from thence ſome knowledge of the original of ſprings; on which I have made ſome remarks, and from whence I have drawn conſiderable advantages for the conſtruction of ciſterns, as I have related in the memoir which I read to the publick aſſembly of the academy, the 18th of *April*, 1703. But as we are perſuaded by moſt obſervations, that it does not commonly rain but when the air becomes lighter, which is known by the falling of the quickſilver in the tube of the barometer; I thought that I ought to add to the obſervations of the rain, thoſe of the barometer, and relate at the ſame time the height of the thermometer, to know what has been the degree of heat or cold, when there has been more or leſs plenty of rain. I have compared theſe different heights of the thermometer, with thoſe wherein it always remains in the bottom of the caves of the obſervatory, which I look upon as the mean heat and the mean ſtate of the thermometer, filled with ſpirits of wine, which I make uſe of, and this height is 48 degrees.

---

* Jan. 9, 1704.

This is the quantity of rain water which fell at the obſervatory, each month of the year 1703, which is meaſured by the height which it would have, if none of it was waſted or evaporated. I have already related in other memoirs of this kind, the manner in which I make theſe obſervations; therefore will not here repeat it. And tho' theſe obſervations have been made daily, I have thought it ſufficient to give each month's reſult thereof, with ſome remarks on this ſubject, and principally of the winds which governed in *January.* The height of the rain water was 9 lines ½, almoſt the whole of which fell near the beginning of the month, with a weſterly wind, inclining ſometimes to the S. and ſometimes to the N. At the end of the month the wind was almoſt always to the N. and without rain.

In *February* there were 14 lines and ¼ of water; the wind was ſomething changeable, but the greateſt part of the month it was ſoutherly.

In *March* it rained but 4 lines, tho' the wind was almoſt always towards the W. between the N. and the S.

In *April* there fell 16 lines and ¼ of water, diſtributed pretty equally throughout the whole month, the wind being almoſt always in the S. inclining to the W. and ſeldom towards the N.

In *May* I found 34 lines and ¼ of water; the governing wind was W. which ſometimes changed to the S. but more commonly to the N.

In *June* there fell but 23 lines of water, the wind being almoſt always in the W.

In *July* it rained 28 lines $\frac{1}{4}$, which fell at the beginning, in the middle, and at the end of the month. When it rained, the wind was almoſt always in the W. inclining to the S. and N. and in the intervals it was pretty often in the N. and a little in the E.

In *Auguſt* the rain furniſhed 23 lines $\frac{1}{2}$, of which 13 lines $\frac{1}{2}$ fell the 12th day of the month, beginning with a little ſtorm, the wind being E. S. E. the wind was almoſt always in the N. and ſometimes inclining to the E. and W.

In *September* the whole height of the rain water roſe to 20 lines $\frac{1}{4}$, which was diſtributed thro' the whole month. The prevailing wind was S. W.

In *October* I got 17 lines of water, which fell each time in little quantities throughout the courſe of the month. The wind was almoſt always in the W. inclining to the S. and ſeldom in the N. or E.

In *November* I collected but 13 lines of water, which fell at the beginning and towards the end of the month, with a S. wind. From the 4th of the month to the 19th, it did not rain, the wind being almoſt always in the E. and ſometimes to the N.

In *December* there fell but 3 lines and $\frac{3}{4}$ of water, but there were many fogs. In two thirds of the month, towards the end of it, there was no rain, tho' the wind was pretty often towards the W. except on the laſt days, when it was about the E.

The ſum of the rain water of the whole year was 208 lines $\frac{1}{4}$, or 17 inches 4 lines $\frac{1}{4}$, which is a little leſs than the common, which is 19 inches; ſo that we may ſay, that this year is one of the dryeſt of theſe countries.

The four months of *May*, *June*, *July*, and *Auguſt*, gave more water than the other eight together, which commonly happens, tho' there have been no conſiderable ſtorms.

The little ſnow which fell in the beginning of this year, is not worth being conſidered. We ſee thereby, that it is not the great quantity of ſnow which makes the earth more fruitful, as is commonly believed, ſince this year has produced much corn and fruit. It is true that ſnow, continuing long on the ſurface of the earth, may keep the ſalts therein, which throw themſelves out of it continually, and which entering the earth again, when the ſnow melts, may make it more fruitful; but there may alſo be rains which may have the ſame effect, if they are impregnated with the ſame ſalts.

The cold was not conſiderable throughout the months of *January* and *February*, wherein it commonly is the greateſt, for my thermometer did not fall to 26 degrees; and I have obſerved, that it does not begin to freeze till this thermometer is at 32 degrees; from whence alſo we may ſee, that there have been no conſiderable froſts. In the laſt months of this year the cold was not ſo great as at the beginning.

If the cold was not conſiderable this whole year, the heat was alſo but moderate, and of little continuance; and I find that the hotteſt days were the 27th of *May*, the laſt days of *July*, and the firſt of *Auguſt*, wherein the thermometer was near 60 degrees. The hotteſt day was the 12th of *Auguſt*, in which the thermometer roſe to 64 degrees. Theſe obſervations are always made near the ſun-riſing, which is the time wherein the air is cooleſt; and between two and three in the afternoon, the heat of the day is greateſt. I have obſerved during the ſummer, that between two and three in the afternoon, the thermometer riſes 10 or 12 degrees more than in the morning at the time of the ſun-riſing, tho' this thermometer is always in the ſhade.

The 28th of *April*, the thermometer having been in the morning at 42 degrees, and the barometer at 27 inches 3 lines $\frac{1}{2}$, there was a ſtorm and pretty loud thunder; and I was told, that near *Villeneuve St. George*, there fell on the earth a very great flame, which frightened thoſe who lay near it, but which did no hurt to thoſe who were in the place where it fell.

As to the barometer, it was at the higheſt on the 10th of *December* in the evening, at 28 inches, 4 lines and $\frac{1}{6}$, at the height of the great hall of the obſervatory, and in the whole month of *December*, the barometer always kept up very high; nor did it rain but 3 lines $\frac{1}{4}$, and this is a confirmation of what is generally obſerved, that it very ſeldom rains when the barometer is raiſed higher than its mean ſtate. There alſo happened very near the ſame thing in *March*, when it did not rain more than 4 lines; but the barometer was not quite ſo high as in *December*.

The third of *January* the barometer was the loweſt in the year at 26 inches, 6 lines $\frac{1}{4}$, with a little rain, and without ſtorms, as it often happens when it is very low. Thus the difference between the greateſt and the leaſt height of the barometer, was this year 1 inch, 9 lines and $\frac{1}{2}$, which is a little more than the common, which does not exceed 1 inch, 6 lines.

I obſerved on the 18th of *December*, the declination of the needle from 9° 6′ N. towards the W. with a needle of 8 inches length. This needle is very well made, and is ſhut up in a wooden box of a ſquare figure; and to make the obſervation, I always place the ſide of this box in one and the ſame part of a pillar, in the lower ſtone gallery of the obſervatory, whoſe poſition in the meridian line I am certain of, by very exact obſervations on the centre of the ſun's paſſage in the meridian.

II. *New obſervations on the inſects of orange-trees, by M.* de la Hire *.

I had formerly examined what is called the grain of cochineal, and I had given a memoir of it to the academy, in which I related at length, all the diſcovery I could make of them by their figure, after having ſoaked them; and among other things I had obſerved, that it was a little inſect, of which nothing remained entire but the belly, which was covered with ſcales; but no part of the body near the head was to be ſeen, nor any feet, which I imagined had been dry'd up and reduced to duſt.

It came then into my thoughts, whether the little inſects on orange-trees were not the ſame with the cochineal; for the figure of the belly appeared to me much like them, and theſe inſects feeding on the juice of the red fruits of *Opuntia*, where they gather cochineal, might give them the red colour and ſtrong dye, with which they are filled.

I had often obſerved, that in bruiſing with the fingers the inſects on orange-trees, they remained dyed of a reddiſh colour, which ſticks pretty faſt to the skin, tho' theſe animals feed only on the juice of green leaves and ſtalks of the tree; and this is what perſuaded me that there was great likelihood in my conjectures; that if theſe inſects fed on the juice of the red fruits of the *Opuntia*, they might give a very ſtrong red tincture; and what more confirmed it was, that I knew thoſe who had eaten of theſe fruits, had made urine as red as blood.

---

* March 8, 1704.

To ſucceed in my deſign, as I had ſome plants of *Opuntia*, which had very red fruits upon them, I placed them underneath, and very near ſome orange-trees, whereon were many inſects not yet hatched. I alſo broke ſeveral of the bags which contained the eggs, and ſpread a great quantity of them over all the *Opuntia*, hoping that ſome of theſe animals would faſten upon it. I obſerved every day, with a great deal of care, both the leaves and fruits of the *Opuntia*, ; and at laſt I perceived one day a very great quantity of little white inſects, which ran with great ſwiftneſs upon the *Opuntia*. I alſo looked on the orange-trees, and in like manner found them proportionably there. I then made no doubt but that theſe little inſects were hatched from thoſe eggs. A little time after, all theſe inſects fixed on the orange-trees, round the branches and under the leaves, and they forſook the *opuntia*, on which there remained none of them, either on the leaves or fruits. Thus I concluded, that theſe inſects of the orange-trees, tho' in appearance ſomething like the cochineal, could not find on the *opuntia* a food ſuitable to them, as on many other plants, and that theſe were not the ſame.

However, my enquiry was not altogether uſeleſs to me; for I then knew that the inſects on orange-trees, from their being hatched till a certain bigneſs, to which they arrived in a little time before they fixed themſelves, might couple, and be in a condition of laying fruitful eggs, at a time far diſtant from that of their coupling, which is not till eight months after; and what ſeems yet more extraordinary, is the great growth of theſe inſects after they are fixed and ſettled, till the time of their laying; for they become 20 or 30 times greater than they were before, and their outward figure being changed, they now appear only like a pretty long piece of tortoiſe-ſhell.

It were to be wiſhed that ſome of the ſeeds of cochineal could be conveyed into the ſouthern parts of *Europe*, as *Sicily* and *Spain*, where the *opuntia* may eaſily be brought; for I make no doubt but that cochineal may there be ſufficiently cultivated, to know perfectly the nature of it, without being obliged to refer to the relation of clowns and ſlaves, who look on the productions of nature only with regard to the profit which it brings to them.

III. *That all the barometers, double as well as ſimple, which have been hitherto conſtructed, act not only by the greater or leſs weight of the air, but alſo by its greater or leſs heat; and the means of preventing this defect for the future in the conſtruction of double barometers, and of correcting the error in the uſe of the ſimple barometers, by M.* Amontons *.

It is proper in the firſt place to relate the particulars of ſome experiments, in order to deduce afterwards from thence, if poſſible, a conſtruction which may prevent the alteration cauſed by heat in the weight of quickſilver, with which common barometers are filled.

---

* June 18, 1704.

Ex-

Experiment I.

The thermometers, of which mention is made at the end of the *connoiſſance de tems* of 1704, being at 54 inches, 5 lines, an areometer was filled with quickſilver, in which entered 18 ounces, 7 drams, 63 grains. After having emptied the areometer, it was filled with ſpirit of wine, and there entered 1 ounce, 1 dram, 28 grains. The quickſilver, ſpirit of wine, and thermometer had been a conſiderable time, as ſeveral days, in the ſame place near one another.

It follows from this experiment, that the weight of the quickſilver is to that of the ſpirit of wine in an equal maſs, near as $16\frac{1}{4}$ to 1, when we do not try it either in great cold or great heat.

Experiment II.

The ſame thermometers being at 54 inches, 11 lines, a little common thermometer glaſs was filled full of quickſilver, the whole which entered therein was 757 grains : the bigneſs of the tube was ſuch, that, in the length of 11 lines, it contained 18 grains. At this rate, a tube of like bigneſs, and of 36 inches, 6 lines $\frac{2}{3}$ long, would have contained the 757 grains of quickſilver. The thermometers being fallen to 50 inches, 11 lines, the little quickſilver thermometer fell juſt 2 lines ; from whence we muſt conclude, that from the great heat to the great cold of our climate commonly taken, that is to ſay, in the time when my thermometers ran from 50 to 58 inches of their graduation, the bulk of the quickſilver augments by about $\frac{1}{115}$ part of what it had in the great cold, and that in equal bulks the weight of it alſo diminiſhes in great heat $\frac{1}{115}$, from that which it had in the great cold.

Expe-

Experiment III.

The thermometers being at 54 inches, ſome ſpirit of wine was put into a glaſs tube, ſealed at one end: it occupied in this tube 32 inches, 4 lines in length; the other end of the tube was afterwards ſealed, and it was left for the experiment. The thermometers being fallen to 50 inches, the ſpirit of wine in the tube was ſunk 7 lines $\frac{1}{4}$; from whence it follows, that from the great cold to the great heat of our climate commonly taken, the bulk of the ſpirit of wine augments about $\frac{1}{27}$ of that which it had in the great cold.

It alſo follows from theſe three experiments, that in the great cold of our climate, the weight of the quickſilver is to that of the ſpirit of wine near as 16 to 1.

This being ſettled, if we ſuppoſe that in the great cold the ſpace between the ſurfaces of the quickſilver of the two boxes of the double barometer is 28 inches, 8 lines; one of theſe inches will counter-balance, or be an *equilibrium* to 16 inches of ſpirit of wine, and the top of theſe 16 inches of ſpirit of wine, will mark for that time on its tube the place where the atmoſphere equals the remaining 27 inches, 8 lines.

By taking the top and bottom of this point of the equal parts of 16 lines, each of theſe parts will be analogous to the lines of the quickſilver of the ſimple barometer, that is, the ſpirit of wine of the tube being at the firſt diviſion under that which marks 27 inches, 8 lines, will ſhew that the air then weighs 27 inches, 9 lines, and only 27 inches, 7 lines, when the ſpirit of wine ſhall be at the firſt diviſion above that which marks 28 inches, 8 lines. Mean while it muſt be obſerved that each of theſe parts of 16 lines ought to be diminiſhed $\frac{1}{18}$ of a line, if the aperture of the tube, which contains the ſpirit of wine, is half that of a line, and the diameter of the box an inch, the reaſon of which is, that the ſpirit of wine which enters into this tube cannot go out of the box, without making the quickſilver fall in a quantity which equals $\frac{1}{18}$ of a line; which makes a difference of $\frac{1}{9}$ of a line in the height of the quickſilver to each part, and there muſt be $\frac{1}{9}$ of ſpirit of wine to be an equilibrium to $\frac{1}{9}$ of quickſilver.

The cold ſuppoſed to be ſtill the ſame, and the barometer being thus regulated, it is evident, that it will exactly mark all the changes which ſhall happen in the weight of the atmoſphere, with this advantage over ſimple barometers, that it will mark them at leaſt 14 times more ſenſibly: but in the great heats of our climate, theſe 28 inches, 8 lines of quickſilver, which in the great cold made an *equilibrium* with the weight of the atmoſphere, will weigh $\frac{1}{115}$ leſs, and ought conſequently in order to continue to counter-balance the ſame weight of air, to be augmented about 3 lines, which are very near the $\frac{1}{115}$ of 28 inches, 8 lines, without which the ſpirit of wine will ſink in its tube 48 lines *minus* $\frac{1}{18}$ of a line, that is to ſay, a little more than 3 inches $\frac{1}{4}$.

This augmentation of 3 lines to the height of the column of quickſilver, cannot be made till the ſurface of the quickſilver of the inferior box falls a line and half, for then this line and half of quickſilver being driven into the ſuperior box, will make one total height of quickſilver of 28 inches, 11 lines, between the ſurfaces of the quickſilver of the two boxes. Now it is neceſſary, in order to hinder this falling of the quickſilver in the inferior box, from cauſing any change in the height of the ſpirit of wine in the tube, that the part of the ſpirit of wine, which is in the inferior box, ſhould dilate itſelf ſufficiently to fill this ſpace of a line and half, which the quickſilver abandons; which will neceſſarily happen, if there is given to that part of the box which contains the ſpirit of wine, an equal capacity with that of a cylinder of the ſame diameter as the box, and of 40 lines ½ of height, ſince theſe 40 lines ½ contain 27 times 1 line and half, and the ſpirit of wine, by the third experiment above related, augments its bulk $\frac{1}{27}$ from great cold to great heat.

It now remains to conſider the changes which heat may make on the ſpirit of wine contained in the tube, according as it may be at different heights, and as the degrees of heat vary.

Firſt, it is now very certain, that while the weight of the atmoſphere ſtops the ſpirit of wine at the bottom of the tube which contains it, whatever change happens to the heat of the air, the ſpirit of wine in the tube will not change its ſituation, and that the whole action of the rarefaction of the liquor, will be made on the ſide of the ſuperior box.

It is alſo very evident, that in great cold, whatever height the ſpirit of wine may have in the tube which contains it, it will always mark preciſely the augmentation or diminution of the weight of the atmoſphere, ſince it is ſuppoſed to be in the ſtate of great cold, that the barometer has been regulated.

It is then only the different heights of ſpirit of wine in the tube, except the time of great cold, which can make any alteration in the exactneſs of this barometer; and though this alteration in the greateſt heights of the ſpirit of wine, in times of the greateſt heats, cannot at moſt exceed 14 lines, and that it is very inconſiderable at all other times, wherein the heat is leſs; yet this is the way to correct it, when it is neceſſary to be exact in the obſervations.

If * in the tube, which contains the ſpirit of wine, it roſe therein thro' the little weight of the atmoſphere, in the time of the great heats, to the height of 28 inches, there would then be on this height of 28 inches one inch to be corrected, becauſe then theſe 28 inches would weigh no more than 27 inches, in the time of great cold. Therefore if they take this tube of 28 inches for one of the ſides, about the right angle of a rectangular triangle, and at this height of 28 inches, a line is drawn of an inch perpendicular to the tube, which makes the other ſide of the right angle of the ſaid triangle; this laſt ſide being divided in ſo many equal parts, as the thermometer contains degrees from winter to ſummer, and numbered in like manner, for example, in 8 with the ſame figures, and that from each of theſe parts the right lines are carried to the extremity of the other ſide, ſo that they divide the triangle into 8 equal triangles, there will be no more to do than from all the diviſions of this firſt ſide to carry lines parallel to the ſecond ſide: theſe parallels will each be divided into as many parts, as that by the lines carried from its diviſions to the extremity of the firſt ſide, and all theſe diviſions will be analogous to the degrees of the thermometer, and will indicate how the liquor ſhould be corrected, that is to ſay, how much of its height ſhould be diminiſhed.

---

* Plate IV. Fig. 1.

Example.

The thermometer being at 56 inches, the liquor in the barometer at 27, let there be taken from the height of the liquor a quantity equal to that part of the parallel 27, which is contained between the lines 50 A and 56 A, and thus of others.

As to the ſimple barometer, the whole extent of its march being bounded in a ſpace too little, for ſuch a ſcale as the preceding to be of any uſe to correct it in ſuch a manner as is neceſſary, the following table may be uſed, which ſhews how much a column of quickſilver of 28 inches, 9 lines, will lengthen or diminiſh at all the degrees of heat indicated by my thermometer.

This increaſe or diminution is expreſſed in this table by $\frac{1}{32}$ of a line. Thus, for example, over-againſt 55 inches, 5 lines, is to be found 65, which means that at the time when my thermometers mark 55 inches, 5 lines, there muſt be diminiſhed from the height of the quickſilver of the ſimple barometer an equal quantity to 2 lines $\frac{1}{32}$ of a line.

It ſhould alſo here be noted, that though 28 inches, 9 lines, are not the mean height of the ſimple barometer, this height being more commonly 27 inches, 6 lines; yet nevertheleſs this table may very profitably be made uſe of without the fear of falling into any ſenſible error.

A table of the heights of quickſilver, which muſt be added or taken from that of the ſimple barometer, according to the different degrees of heat indicated by my thermometer.

Degrees of the thermometer.			Heights to be corrected.	
49 inches	0 lines	add	12	32ds of a line.
49	1		11	
49	2		10	
49	3		9	
49	4		8	or ¼ of a line.
49	5		7	
49	6		6	
49	7		5	
49	8		4	
49	9		3	
49	10		2	
49	11		1	
50	0		0	
50	1	take away	1	
50	2		2	
50	3		3	
50	4		4	
50	5		5	
50	6		6	
50	7		7	
50	8		8	or ¼ of a line.
50	9		9	
50	10		10	
50	11		11	
51	0		12	
51	1		13	
51	2		14	
51	3		15	
51	4		16	of ½ a line.
51	5		17	
51	6		18	

De-

Degrees of the thermometer.			Heights to be corrected.	
51 inches.	7 lines —	take away	19	32ds of a line.
51	8		20	
51	9		21	
51	10		22	
51	11		23	
52	0		24	or ¼ of a line
52	1		25	
52	2		26	
52	3		27	
52	4		28	
52	5		29	
52	6		30	
52	7		31	
52	8		32	or 1 line.
52	9		33	
52	10		34	
52	11		35	
53	0		36	
53	1		37	
53	2		38	
53	3		39	
53	4		40	or 1 line ¼.
53	5		41	
53	6		42	
53	7		43	
53	8		44	
53	9		45	
53	10		46	
53	11		47	
54	0		48	or 1 line ½.
54	1		49	
54	2		50	
54	3		51	
54	4		52	
54	5		53	
54	6		54	
54	7		55	

De-

Degrees of the thermometer.		Heights to be corrected.	
54 inches.	8 lines	— take away 56	32ds or 1 lin. ¾
54	9	57	
54	10	58	
54	11	59	
55	0	60	
55	1	61	
55	2	62	
55	3	63	
55	4	64	or 2 lines.
55	5	65	
55	6	66	
55	7	67	
55	8	68	
55	9	69	
55	10	70	
55	11	71	
56	0	72	or 2 lines ¼.
56	1	73	
56	2	74	
56	3	75	
56	4	76	
56	5	77	
56	6	78	
56	7	79	
56	8	80	or 2 lines ½.
56	9	81	
56	10	82	
56	11	83	
57	0	84	
57	1	85	
57	2	86	
57	3	87	
57	4	88	or 2 lines ¾.
57	5	89	
57	6	90	
57	7	91	

De-

Degrees of the thermometer.			Heights to be corrected.
57 inches.	8 lines	— take away	92 32ds of a lin.
57	9		93
57	10		94
57	11		95
58	0		96 or 3 lines.
58	1		97
58	2		98
58	3		99
58	4		100
58	5		101
58	6		102
58	7		103
58	8		104 or 3 lines ¼.
58	9		105
58	10		106
58	11		107
59	0		108 or 3 lines ½.

IV. *The hiſtory of the* Formica-leo, *by M.* Poupart [*], *tranſlated by Mr.* Chambers.

The *formica-leo* [†] is an inſect which reſembles the ſpider, both in its diſpoſition, the figure, and ſoftneſs of its body, and its manner of ſpinning; it alſo bears ſome relation to the palmer, or *millepes*, and at firſt ſight, one would even miſtake it for that animal; 'tis of a dirty browniſh colour, and beſet with black ſpecks, which when viewed thro' a magnifier, appear like ſo many prickles or quills of a porcupine. Its body is inveſted with *annuli*, which render it all wrinkled; it has 6 feet, 4 of them faſtened to its *thorax*, and 2 to a long projecture, which may be taken for its neck; its head is very ſmall and flat, its 2 horns hard and hollow, about 2 lines long, and ſomewhat thicker than a hair, being hooked at the end like a cat's claws; by the microſcope they appear much like the horns of a large beetle, called the *ſurvus bolens* *; at the baſe or bottom of each is a little black eye, which is very ſharp ſighted, as appears from the animal's taking the alarm, and flying at the leaſt appearance of an object.

---

[*] Aug. 30, 1704.

[†] Plate IV. Fig. 2 and 3.

It is called the *formica-leo*, q. d. *ant-lion*, from its living chiefly upon ants, which have the fortune to fall into its ambuſcade; tho' this ſhould hardly intitle it to the denomination of a lion; its artifice is rather that of a fox, and it would be more proper to call it *formica-vulpes*. The creature is obliged to be very grave and wary, as having nothing to live on but a few inſects, which fall into its clutches, but eſpecially ants, for all the little animals with wings eſcape its attempts, and moſt of the reſt are either too big or too hard ſkinned to be penetrated and faſtened with its horns.

Its way of catching inſects is as follows, it uſually encamps under the foot of an old wall, to be ſheltered from the rain, and the place that it pitches on muſt be a fine dry ſand, that it may be able to make a pit or trench therein, of the form of a tunnel.

To dig only a ſmall pit, it bends its hind-part, which terminates in a point downwards, and with this, as a plough-ſhare, opens the ground, walking backwards; and by little jirks, when arrived at a certain depth, it throws up the ſand a great height with its head, and its ditch is finiſhed; but to make a deep ditch, it firſt draws a large circle, which is to be the top of the tunnel *; then it digs under the ſand, at each ſtep it makes, which is always backwards; in its deſcent, it deſcribes a ſpiral line, which at length terminates in the point or tip of the tunnel or hollow cone.

---

* *So it is written in Mr.* Chambers'*s manuſcript, it ſhould be* cervus volans, *or* flying ſtag.

† Its head is very fit to heave off the ſand, as being flat and its neck very long, when not drawn in, by which means it is enabled to give violent toſſes; and accordingly I have ſeen it throw the *animalcula* it has ſucked, to the diſtance of $\frac{1}{2}$ a foot from its den.

The pit being finiſhed, the animal places itſelf aſide of its bottom, ſhewing nothing but its two horns, which protend into the tip or bottom of the pit; lying thus perdue, if an ant or other like inſect happen to paſs by the edge of the pit, it neceſſarily tumbles ſome of the looſe ſand into the bottom, which is a ſignal to the *formica-leo* that there is prey at hand; upon this it falls a throwing up ſand with its head upon the poor ant, in order to bring him tumbling down into the bottom of the cave between his horns, for the creature never runs after its game; and if it happen that the ant do not fall plum between his horns at firſt, but that perceiving the ſnare laid for her, ſhe begins to clamber out of the pit, the mobility of the ſand ſometimes lets her drop again; or if this fail, the *formica-leo* being ſtill upon the watch, throws up new ſand to bring her down a ſecond time; and if ſhe now fall between his horns, he gripes her faſt, plunging them deep into her body, and proceeds to ſuck her ſo long as any moiſture remains; this done, the carcaſe is thrown out, and if any damage had been done to the pit, the animal makes it up again for a ſecond chaſe.

---

* Fig. 5. † Fig. 4.

The *formica-leo* would rather die of hunger, than go ſeek its living like other inſects, nor is it out of lazineſs as might be ſuppoſed, that it plays this fox's game, for it has no other, being unable to walk except backwards, and by little jirks. It lies night and day upon the ſcout under the ſand at the bottom of its den, for that being unable to go out in queſt of its game, it can only wait till chance brings it home to it, which happens very ſeldom, ſo that it is obliged to ſupply with perſeverance, what nature has denied it by agility.

Indeed the reaſons I am going to produce, ſeem to make all theſe ſtratagems of little effect to the ſubſiſtence of the animal, which one would imagine only caught inſects out of diverſion and wantonneſs, like the ſportſman who only goes to the chace for the pleaſure of purſuing for.

Firſt, he only gripes the inſects with the extremity of his horns, which do not ſeem to be perforated at the end, ſo that 'tis hard to conceive how he ſhould ſuck their juices by that part.

Secondly, upon viewing him with a magninifier, there is no ſign of his thruſting out a proboſcis to ſuck the little animals he catches, as other inſects do; and there is always a conſiderable diſtance between his head and the unhappy animal, which never comes nearer him han the points of his horns.

Thirdly, ſeveral *formica-leo's* have been ſhut up in a cloſe box for the ſpace of ſix months, yet have lived as well as others which were ſupplied with flies, and even made their pits, and overwent all the natural changes hereafter mentioned, which ſeems to intimate that the *formica-leo* may live without food.

But if it be conſidered, that its horns grow again after they have been cut; that it dwindles and grows leſs when it has no food; that after catching a ſingle inſect it appears viſibly bigger than before; and that by ſucking a fly two or three hours, it renders it ſo dry and friable, that it may be crumbled into powder between the fingers. It muſt be allowed, that altho' the animal may live without our perceiving how, or by what part, it takes in its food; yet food it does take.

I am perſuaded, therefore, that its horns are to be conceived as two ſyringes, wherewith it pumps the juice of animals; in effect, viewing them with a microſcope, which magnifies a great deal, I obſerved a tranſparent, membranous body, running the whole length of the cavity of the horn, which may well enough ſerve for the piſton, or ſucker of the ſyringe.

The *formica-leo* being arrived at a certain age, and preparing for a renovation, in order to appear under a new form, it ceaſes to make any more pits, but continues to plough the ſand, which is found all furrowed over with irregular tracks.

[*] After ploughing a long time, it neſtles under the ſand, where it makes a hollow ball, and ſhuts itſelf therein while it changes its figure; this ball conſiſts of ſilk, which it ſpins from its hind-part, much as ſpiders do of a *gluten* or viſcid matter, iſſuing from all parts of its body, and of a ſand which it finds in the place.

To make the ball, it turns ſlowly around as on a centre, vibrating its hind-part to the right and left, and making it touch the ſand, that the ſilk may ſtick thereto, either by its catching on the inequalities of the grains of ſand, or adhering to the glutinous matter it may be ſmeared with; but however this be, the ſands ſtick ſo cloſe to the ſilk, that 'tis difficult ſeparating them, even though they be vigorouſly ſhaken while the work is yet ſoft.

---

[*] Fig. 6.

This ſilk is incomparably finer than the common ſilk, being ſcarce perceivable without the help of a microſcope; to obſerve it well, you muſt take it up ere the work be quite finiſhed, in which caſe it will be found ſoft and cottony, as not being yet hardened by the *gluten*, which oozes at a very ſlow rate from the pores of the animal. Upon taking up this ſilk with the point of a needle, one ſees a ſpace between the grains of ſand, but without ſeeing the ſilk that connects them, unleſs a magnifier be applied.

'Tis impoſſible, without a ſtratagem, to ſee how theſe little animals ſpin their ſilk, and builds their cells, by reaſon they always work under the ſand. In order to this, their work is to be taken up once, and again ere it be finiſhed; upon which they begin again, till at length they are grown ſo feeble, that being no longer able to hide themſelves under the ſand as uſual, they begin ſlowly to ſpin their ſilk on the ſurface thereof, after the manner above-mentioned.

The *formica leo* having wrought a long time, finds itſelf in the middle of a large ſoft ball, which hitherto only conſiſts of ſilk and ſand intermixed; but it hardens by little and little, in proportion as it imbibes the viſcous matter oozing from the body of the animal, which penetrates the cell on all ſides.

What chiefly convinced me of a viſcid moiſture tranſuding from the body of this animal is, that there were ſeveral grains of ſand ſticking on the neck of one of my *formica leo's*, and forming a little kind of rock thereon; while this clog was on its neck, it ceaſed working at its pit, as being obſtructed in the motion of its head; but upon breaking it with nippers, the animal immediately went to work.

When the *formica leo* is incloſed in its cell, it lines the inſide thereof with new pure ſilk, unmixed with ſand, forming a cloſe kind of pearl coloured web, wherein the animal lies at eaſe, with its head between its legs. At firſt ſight, one would take this web for a dry *gluten*, ſeparated from the body of the animal; but if this were ſo, it would break with bending and folding, which it does not; add, that this lining is a continuation of the cell, or at leaſt is faſtened ſo cloſe thereto, that there is no parting them without deſtroying the ball. Steeping ſome of this ſilk ſeveral days in water, I found it did not diſſolve as a *gluten* would have done; tho' it loſt its fine colour; an argument that the little *gluten*, which was mixed with the ſilk, and perhaps gave it its colour, had diſſolved, and left the ſilk alone. This ſilk is much like that which certain ſpiders hang on the leaves of trees, to ſerve them as a lodge or *nidus*, to lay their eggs in, but is thicker than ſuch ſpider ſilk.

That the *formica-leo* does not begin to line in the inſide of its cell, till after it is finiſhed, appears hence, that opening it before it be hardened, we find none of the lining above-mentioned.

My *formica leo's* remained 6 weeks or 2 months, in their cells, before they changed into maggots, tho' this time of their continuance is by no means regular and certain; they were diſpoſed with the head between the legs, for the ſake of roundneſs, and that they might take up the leſs room, and accommodate themſelves to the concave figure of the ball as much as poſſible.

When the time was come for changing their figure, they began to put off their firſt ſkin, to which the horns, eyes, and hair were faſtened. This ſkin was now like a ſhriveled clue, whitiſh on the inſide, having an aperture all along the belly, at which the inſect hereafter-mentioned had iſſued.

The *formica-leo* having quitted its ſkin, appears under the form of a maggot*, with 4 membranous wings, 6 feet, 2 large horns or hollow, ſoft *antennæ*, 2 black eyes, and 2 pinchers in form of ſaws, which ſerve it for teeth; the whole being about a quarter of an inch long. This maggot remains ſometimes in its retirement e're it comes forth in a new form, tho' the preciſe time is not ſo eaſy to determine, by reaſon the *formica-leo* it ſprings from lies hid before the metamorphoſis.

When the maggot is diſpoſed to quit its cell, it eats a little roundiſh hole therein, which however does not appear round, by reaſon the piece uſually remains faſt on one ſide †, which renders the paſſage ſo narrow, that half the maggot remains in the cell, and the other half out. In this ſtate, the maggot is no living thing, being only a tranſparent membranous caſe, with horns or *antennæ*, eyes, teeth, wings, feet, &c. which are the caſes of the like parts of a beautiful fly, called a *maid* ‡, which iſſues hence by a cleft made on its back near the head.

---

* Fig. 7, 8, and 9.   † Fig. 10.

‡ So Mr. *Chambers* tranſlates it: the original is *demoiſelle*; it is an inſect, called, in *Engliſh*, *adder-bolt*, or *dragon-fly*; ſee Vol. I. pag. 94, of this abridgment.

This fly is 15 or 16 lines long; its wings at firſt are only 2 lines, by reaſon having been ſet in caſes or frames of that length, they have aſſumed the figure and ſize thereof; but we find them folded in 2 plaits, which, in two minutes time, open and ſtretch out, and thus grow longer than the body; after quitting its caſe the *maid* remains ſometime on its feet motionleſs, to let its wings dry, that it may take its flight ‖, and enjoy a happier ſtate of life than that it led under the ſkin of the poor *formica-leo*.

While the *maid* is incloſed in its maggot, it cannot attain to above 3 lines long, by reaſon the maggot itſelf is no bigger; but it is no ſooner at liberty, than it lengthens and extends to upwards of 15 lines. This ſudden expanſion ariſes hence, that the *maid*, while in its caſe is folded and contracted like a cat-cawl, when ſqueezed at the two ends; but after its delivery hence, it recovers all its magnitude like a ſponge, which having been crowded between the fingers, reſumes its dimenſions when let go again.

In the year 1703, the *formica-leos* I had obſerved did not change into *maids*, it being the year following ere that metamorphoſis took place: an indication that theſe animals do not change in the firſt year, and that they muſt be to a certain age ere they be fit for transformation.

When the *maid* is gone, opening the cell the *formica-leo* had lain in, we find it lined as abovementioned, with a gloſſy pearl coloured ſatten, the ſhriveled ſkin or *exuvia* of the *formica-leo*, is always found therein, together with the membranous caſe which immediately incloſed the *maid*; but what is more extraordinary, we ſometimes find an egg *, which the *maid* depoſits there, before it goes. The egg is 2 lines long, 1 thick, and bears ſome reſemblance to a ſlender acorn; its ſhell is hard, and in all reſpects like that of a hen's egg. The ſubſtance contained in it is not fluid, and I have obſerved the eggs to be of different colours at different times; one of them being expoſed ſome days to a hot ſun, the matter within it grew hard and black as ink.

‖ Fig. 11.

Theſe young *maids* ſeem only to lay one egg, for we found but one a-piece in the bodies of ſeveral which had been opened, only one in a cell which another had relinquiſhed, and another *maid* riſing to the top of the box, it was kept in, ſome hours after dropped another; and yet 'tis ſcarce probable, that each *maid* ſhould only lay a ſingle egg, by reaſon there will always be ſome which are not prolifick, and others of them muſt produce males, from whence it is eaſy to infer, that the ſpecies by degrees muſt have totally ceaſed. The precipitancy wherewith theſe maids lay their eggs are a proof, that they do not always wait for the embraces of the male ere they bring them forth; and it may be owing to the ſcarceneſs of ſuch coupling, that the *formica-leos*, and the little *maids* ariſing from them, are ſo rare.

The little ball the *formica leo* is lodged in, is abſolutely neceſſary for the production of the *maid*; for breaking ſome of them, and turning out the *formica leos* naked upon the ſand, they were ready for transformation. They did indeed put off their ſkin; but the *maids* could not get forth from the maggots they were incloſed in, tho' they lived a long time after, and made ſeveral attempts to get forth. One of the chief uſes of this ball is, that by means hereof the *maid* ſlips off the maggot it is inveſted with, in ſqueezing, with ſome difficulty, through the little hole the ſame maggot had made with its teeth.

---

* Fig. 12.

It muſt be obſerved, that the ſeveral kinds of *maids* friſking in the ſummer-time about rivers, buſhes, *&c.* do not all ariſe from this little animal; ſuch as do, have two *antennæ*, which are very ſlender towards the head, and grow bigger and bigger to the end. They have two large eyes on the ſides of their head, and none atop of it, as the other *maids* have. Their belly is not furrowed lengthwiſe, as is found in the reſt; and the tip of their tail is beſet with hairs. Their wings are of an aſh colour, ſpotted with black, without any vivid colours variegating them: ſo that 'tis probable thoſe beautiful flies, which the variety of their colours has occaſioned to be denominated *maids*, with all the ſeveral ſpecies thereof, are of another origin.

There are two other beautiful ſpecies of large *maids*, whoſe origin is certainly very different from thoſe above-mentioned: they ariſe from two water animals, which have no reſemblance to the *formica-leo.*

We ſhall take occaſion hereafter to ſhew, that the animals they ariſe from are real fiſhes; for we have obſerved their gills; and ſhall here, by the way, preſent the figures thereof[*], as well as of the animals themſelves[†].

Explanation of the figures, tranſlated by J. M.

Plate IV. *Fig.* 2. This figure repreſents the *formica-leo*, drawn three times as big as the life, to ſhow how he is briſtled with prickles.

3. The under part of the *formica-leo.*

---

[*] Fig. 15 and 16.

[†] Fig. 13, 14, and 17.

4. The head and neck of the *formica-leo* ſeparated from the breaſt, and drawn much larger than the life, that the ſmalleſt parts may be diſtinctly ſeen.

5. The pit which the *formica-leo* has made for inſects to fall into. He is hid at the bottom, where he lets nothing but his horns be ſeen, which he keeps divided, that he may be quite ready to ſeize the little animals.

6. The lodge in which the *formica-leo* is ſhut up, in order to change its form.

7. The little worm which appears after the *formica-leo* has quitted its ſkin, in which the *demoiſelle*, or *adder-bolt* (11) is incloſed.

8. This figure repreſents the little worm (7) drawn much larger than the life, that its eyes, feet, and wings may be diſtinctly ſeen, which are ſheaths or caſes, in which the ſame parts of the *demoiſelle* are contained.

9. This odd figure, which is drawn much larger than the life, is the little worm which was repreſented in the figures 7 and 8, in its ſituation in the lodge. Its back is bent, that it may ſuit the figure of his lodge, and take up leſs room.

10. The ball, or lodge, of the *formica-leo*, with the worm marked 7, part of which is within and part without; from whence the *demoiſelle* (11) is come out through a crevice made on the back of the worm.

11. This figure repreſents the *demoiſelle*, when ſhe is come out of the worm 7, 8, or 9.

12. The eggs which the *demoiſelles* lay almoſt as ſoon as they are come out of their little lodges, or balls.

13. An aquatic animal, from whence proceeds a great kind of *demoiſelle*, different from that which comes from the *formica-leo*. This little animal is a real fiſh.

14. The underpart of the aquatic animal repreſented in *fig.* 13.

15. A ſort of mask, which covers the head of the aquatic animal marked 13, which are the gills ſeen outwardly.

16. A mask which covers the fore part of the head of the aquatic animal marked 13, which is its gills ſeen inwardly.

17. Another aquatic animal, a little differing from the preceding, from which comes out a great kind of *demoiſelle*, variegated with fine colours.

V. *A diſcourſe on the barometer, by M.* Amontons *.

Among the phyſical diſcoveries of the laſt age, that of the barometer, or of the manner of meaſuring the weight of the atmoſphere, may well hold the firſt place.

The clearneſs and evidence with which we now explain many effects of nature, where we only ſaw obſcurity and uncertainty before this diſcovery, are convincing proofs of it. Hardly any body is ignorant that the effects, which were formerly attributed to the abhorrence of a *vacuum*, had cauſes which were wholly unknown to thoſe very perſons, who were moſt fond of uſing this expreſſion.

Thus, what is very obſcure and impenetrable at one time, becomes the utmoſt evidence in another.

But altho' it be true, that ſince this diſcovery, we have cleared upon this ſubject an infinity of very difficult things, with all the clearneſs that can be wiſhed; we cannot however doubt, but that there yet remains a great number, and that theſe laſt are much more ſo, as they are leſs apparent, and that they do not preſent themſelves at firſt to the mind, as the former did.

---

* Nov. 12, 1704.

In the novelty of the barometer, the ſurprizing effects of the weight of the air alone, have drawn all the attention of thoſe who ſaw them, we ſuffered ourſelves willingly to be prepoſſeſſed, that it was the ſole cauſe of the motion of the quickſilver; and if we reflected, that there is nothing upon which heat does not act, we thought that in this caſe it was ſo little, that it was not worth regarding. We eaſily paſſed over a reaſoning which had nothing new, to admire a ſyſtem, of which the novelty agreeably ſurprized us by its happy ſucceſs; and we had, if I may ſo ſay, eyes only to conſider a multitude of experiments, all of them curious, which preſented and explained themſelves, as of their own accord, without there being need to determine any thing exactly.

In effect, it ſignified little to give the reaſon, for example, of pumps, ſiphons, and almoſt all the other experiments of the gravity of the air, to know that the weight of the quickſilver was not the ſame in ſummer, as in winter. It was ſufficient, that we were aſſured that it was not ſo conſiderable a quantity as to hinder the determining in general the elevation of the quickſilver in the tubes to about 28 inches, and that of water to 32 feet.

But, in fine, theſe apparent and palpable effects of the weight of the atmoſphere being now ſufficiently explained in a general manner, it remains for us to do it in a more particular and exact manner, and to carry our attention to others, which by being more concealed are not leſs uſeful.

The only thing here, that could trouble us, is, that the barometer proper for explaining in general the effect of pumps and ſiphons, becomes faulty and bad when it is uſed, for example, to meaſure the viciſſitude of the weight of the atmoſphere, to determine the height of it, and to level many points of the ſurface of the earth. In the obſervation of the greater or leſs gravity of the atmoſphere, we may find a difference of above 3 lines in the height of the quickſilver, tho' in reality the weight of the atmoſphere has not changed; which proceeds from the effect which the heat produces upon the quickſilver of the barometer, experience having taught us, that a column of quickſilver of 28 inches, 9 lines in winter, and one of 29 inches in ſummer, are of the ſame weight; ſo that the ſimple barometer being carried in the coldeſt time of our climate, from a place above the ſurface of the earth, into another made hollow beneath it, may give 1 line ½ difference in the height of the quickſilver, which we have falſly attributed to the weight of the column of air which was between theſe two places; and if we had a mind to determine by this experiment the height of the atmoſphere, or the difference of the level of two places of the earth, we ſhould run great riſque, of doing both very badly.

The barometers are either ſimple, that is, charged only with quickſilver; or double, that is, beſides quickſilver a 2d liquor is alſo uſed, which is commonly oil of tartar coloured. As for the ſimple barometers, the extent of their motion is very moderate, hardly exceeding 23 or 24 lines, and there is no other way to avoid errors in theſe, than to prepare a table of correction, which ſhews the proportional quantities by which the heat lengthens the column of quickſilver, from winter to ſummer, and that conſequently it is con-

venient

venient to retrench from the heights indicated by the barometer, at the time of the obſervation. For example, my thermometers, that is, thoſe which are deſcribed at the end of the *connoiſſance des Tems de* 1704, and in the memoirs of 1702 and 1703; theſe thermometers, I ſay, marking 58 inches, which is the time of our greateſt heats, there are 3 lines to be retrenched from the height of the quickſilver of the ſimple barometer; 2 lines when theſe thermometers mark 55 inches 4 lines; only 1 line when they mark but 52 inches 8 lines; and 0, or nothing, when they mark but 50 inches; and ſo of the other corrections to be made for all the other degrees of heat between theſe, which we ſhall find by preparing an exact table upon this foundation.

But as to the double barometers, whoſe motions are much more conſiderable, and upon which the heat produces different effects, the combination of which is the cauſe that we cannot eaſily make the corrections by a table; beſides that the perſons, who make uſe of theſe barometers, are for the moſt part but little accuſtomed to theſe ſort of corrections; this is the way that I have uſed, that theſe corrections might be made as it were of themſelves, and without a table.

Theſe barometers are compoſed of two glaſs boxes * A B, which have a communication with one another by a bent tube A C B.

The box A terminates in a point which is hermetically ſealed. The upper half of this box is emptied of groſs air, the other half, the tube A C B, and the lower half of the box B, contains quickſilver. This ball B terminates in a very ſlender tube B D, open at D. The upper half of the box B, and part of the tube B D, contain a liquor which riſes and falls in the tube, according as the atmoſphere is more or leſs light, the quickſilver A C counter-balancing and making always *equilibrium* with the quickſilver C B, the liquor B D and the atmoſphere.

---

* Plate IV. Fig. 19.

All this at preſent ſeems to be almoſt univerſally known, but that which ſeems not to have been yet obſerved by any body, is, that the quickſilver contained in A B, becoming lighter in the ſummer than in the winter, the atmoſphere repels toward the bottom the liquor contained in the tube B D, to 3 or 4 inches, and makes us falſly preſume that the atmoſphere is become thus much heavier, tho' in reality its weight has not been at all changed.

Therefore to prevent this fault, the column of quickſilver A B muſt be lengthened ſufficiently, to replace the weight which the heat makes them loſe, without the liquor in the tube changing its place. For this purpoſe I took a liquor, which eaſily rarefies by the heat, as ſpirit of wine does; I ſubſtituted this liquor to oil of tartar, which is commonly uſed, and which does not rarefy near ſo ſenſibly.

I increaſed the capacity of the box B, which commonly contains this liquor, that it might hold more, and alſo produce a ſufficient rarefaction, to make the quickſilver of the box fall, and by this means lengthen the column of quickſilver A B, which without that would not lengthen at all, altho' the heat had rendered it lighter, becauſe the atmoſphere not preſſing immediately upon the quickſilver of the box B, but upon the liquor in the tube D, it would make this liquor ſink and ſupply by this means the lightneſs of the quickſilver; which, as I have already ſaid, would make us falſly preſume that the atmoſphere is become heavier, altho' it is not at all changed; whereas the liquor in the box B, finding in its rarefaction always the ſame reſiſtance on the ſide of the atmoſphere, ſuppoſing that its weight has not changed at all; and finding leſs from the ſide of the quickſilver rendered lighter by the heat, this liquor employs the whole action of its rarefaction againſt the quickſilver, which it repels and replaces always *in equilibrio* with the atmoſphere, without the liquor of the tube D, upon which the atmoſphere immediately acts, being conſtrained to change place, unleſs the atmoſphere changes its weight; and the whole artifice of this conſiſts only in well proportioning the capacity which contains the liquor to the capacity of the tube of the barometer; for too ſmall a one would not entirely correct the error; and too great a one, by repelling the quickſilver too much, would make the liquor in the rarefaction find at laſt too much reſiſtance from the quickſilver, and be obliged to act on the ſide of the atmoſphere; which would make us falſly preſume that the air was become lighter.

It remains for me to obſerve, that altho' by this means the error that might happen by the greater or leſs lightneſs of quickſilver is corrected of itſelf; there is alſo a ſecond to be avoided, which might be cauſed by the greater or leſs lightneſs of the liquor.

This error is truly not ſo conſiderable as the firſt, and might very well be neglected without any great conſequence; but it will be always better to have regard to it, chiefly in caſes where exactneſs is required; and this might be done by the means of graduation, as I am going to ſay.

We commonly divide this graduation into parts equal among themſelves, which ſignify nothing, and are only to expreſs by their greater or leſs number, that the liquor is more or leſs high, and conſequently that the atmoſphere is more or leſs light, but not how much; and theſe numbers never expreſs the weight of the atmoſphere.

I have therefore judged it moſt proper that theſe parts, altho' much greater, ſhould repreſent the inches and lines that the quickſilver reaches in the ſimple barometer, from the leaſt to the greateſt lightneſs of the atmoſphere.

Thus I divide all my graduation, which is about 28 inches, into 24 equal parts, which expreſs the 24 lines comprehended in the ſimple barometer between 28 inches 4 lines, which is the greateſt gravity that I have known in the atmoſphere, and 26 inches 4 lines, which is the leaſt.

I allow to this graduation about 14 lines in breadth at the top, and only a line and $\frac{1}{4}$ or thereabouts at the bottom.

I divide each of theſe breadths into 8 equal parts, and draw right lines from the diviſions at the top to thoſe at bottom, which make 8 *trapeziums* about 28 inches long.

Laſtly, I cut all theſe *trapeziums* by lines parallel to each other, drawn from the 24 aſcending diviſions, after having numbered theſe 24 diviſions, in deſcending from 26 inches 4 lines, to 28 inches 4 lines, I numbered the 8 lateral diviviſions from 50 to 58.

Theſe 8 lateral diviſions repreſent the 8 inches contained upon the graduation of my thermometer, from 50 to 58, that is, from the greateſt cold to the greateſt heat of our climate, and ſerve to make the correction of the error, that the greateſt or leaſt lightneſs of the liquor of the barometer might cauſe, and that in the following manner.

I firſt conſider at what diviſion my thermometer is; I afterwards take upon my barometer over-againſt the place where it is found, the lateral part contained between the firſt aſcending line of the graduation, and the aſcending line which anſwers to the diviſion that I have obſerved upon the thermometer.

Adding this part to the height of the quickſilver that the barometer indicates, I have the exact weight of the atmoſphere.

Here would be the place to give the reaſon of the particular conſtruction of this barometer, and of its graduation; but as it is deduced from a detail which would be tedious, and which I have already given *; thoſe who would know more of it, may have recourſe to it. I ſhall content myſelf with obſerving that this barometer, beſides its great exactneſs, has alſo the advantage of being almoſt as ſenſible again as the others, and that great care muſt be taken that there does not remain any air in the top of the upper box under the quickſilver.

After what I have juſt ſaid of the effect of the heat upon the liquors that the double barometers are filled with; it remains to examine what its action may be upon the glaſs that contains theſe liquors; and if there is no room to fear that this may alter the indication alſo of the greater or leſs gravity of the atmoſphere, which is not without foundation. For, in ſhort, we know nothing in nature, of all that falls under the ſenſes, upon which heat does not manifeſt its power; thus there is no doubt of its

---

* Pag. 161 of this volume.

acting upon the glaſs, as upon all other things, and of its dilatating it in ſuch a manner, that the capacity of a glaſs veſſel or bottle is greater in the ſummer than in the winter. But the queſtion is, to know if this may be conſiderable enough to cauſe any alteration in the barometer.

Now by many exact experiments, I have found that a white glaſs bottle, pretty thick, of a cylindrical figure, and ſuch as thoſe are, which are commonly ſtopped with a glaſs ſtopper, full of common water, of which the degree of heat meaſured by my thermometer was equal to 54 inches, and which contained about 14 ounces of this water, increaſed its capacity only $\frac{1}{2688}$, when I immerged it in other water, of which the degree of heat meaſured upon the ſame thermometer, was 64 inches: from whence we may judge, that this effect is ſo trifling, that it cannot be ſenſible in the glaſs of a barometer, the capacity of which is nothing near ſo conſiderable, as that of this bottle.

VI. *Extract of a letter from M.* Sarraſin *, *phyſician to the king, in* Canada, *concerning the anatomy of the caſtor, read to the academy, by M.* Pitton Tournefort; *tranſlated by Mr.* Chambers.

The largeſt caſtors are 3 or 4 feet long, and 12 or 15 inches broad in the middle of the *thorax*, and from one haunch to the other; and they are commonly from 40 to 60 *lb.* weight. Their life hardly laſts above 15 or 20 years; as to colour, they are uſually very black in the fartheſt parts of the north, tho' ſometimes alſo white; thoſe of *Canada* are generally brown, but this colour grows brighter as the countries are more temperate, for they are fallow, and even almoſt ſtraw-coloured among the *Ilinois* and *Chaouanons*.

---

* From Quebec, Oct. 15, 1700.

The caſtor, whoſe deſcription we here give, was very black, tho' taken on the edge of a little lake 12 or 15 leagues from *Quebec*; it weighed about 50 pounds.

This animal is every where beſet with 2 kinds of hair, except on the legs, where there is but one, and that very ſhort hair; the firſt kind of hair is from $\frac{1}{4}$ of an inch to two inches long, being continually ſhorter; the nearer it is to the head and tail, it is the thicker, rougher, and more gloſſy kind, being that to which the colour of the caſtor is chiefly owing; viewing it with a microſcope, we find a line in the middle more tranſparent than the reſt, which makes us conjecture it to be hollow.

The other kind of hair is a very fine cloſe down, about an inch long, which helps to keep the caſtor from the cold, and ſerves to make hats and other ſtuffs withal; thoſe skins which have been wore by the ſavages, either as garments or as coverings of beds, are the moſt valued, both as they are ſtripped of their coarſe hair, and as the down left on them being become fat and unctuous, by the matter of inſenſible perſpiration, works ſo much the better; this down, when the animal is alive and works, is preſerved from the mud and dirt, by the other longer and ſtiffer kind of hair.

'Tis not eaſy diſtinguiſhing at firſt ſight whether a caſtor be male or female; all that appears without is an orifice under the tail, whereby both kinds of excrements are evacuated. The parts which diſtinguiſh the ſex are hid under the muſcles, and to make a judgment thereof, the ſkin and fleſh between the *os pubis* and this orifice, muſt be taken up, upon which the yard will be felt, which is hard and about the length and thickneſs of a finger.

Under the ſkin we find a layer of fat, which under the belly is 8 or 10 lines thick, and extends from the jaws to the tail, but it diminiſhes gradually in approaching towards the back where there is none at all. A ſecond layer of fat we find between the two oblique muſcles of the belly, but this is hardly ¼ of an inch thick, the viſcera have ſcarce any, and the caul, tho' as large as in other animals, only weighs 3 or 4 ounces.

The muſcles of the caſtor are all very ſtrong, and ſeem bigger than needs be, conſidering the ſize of the animal. The fibres of the cutaneous muſcle have very different directions; thoſe covering the back, from the hips to the neck, are ſtrait; and ſo big, that the muſcle is here almoſt an inch thick; the fibres ſituate aſide of theſe, recede gradually from hence, and make a much leſs bulk; they deſcribe almoſt ſemi-circles, which deſcending on the pectoral muſcles, the *ſternum*, and all along the ſtrait muſcles, unite by an *aponeuroſis*, in ſuch manner, that they inveſt the whole animal. Part of theſe fibres cover the hips, after which traverſing each other on the *os pubis*, they deſcend and form a kind of tiſſue in manner of a mat, which covers not only a very conſiderable bundle of fibres, but alſo the *ſphincter* of the *anus*.

From the inner ſurface of the mat abovementioned, about 12 or 15 lines below the *os pubis*, ariſe two branches of fleſhy fibres, about the thickneſs

neſs of a finger, which aſcend to the inſertion of the ſtrait muſcles, and are faſtened thereto. Of that part of this muſcle which covers the back, and whoſe fibres are ſtrait, a ſtrong *aponeuroſis* is formed, which inveſts all below the thighs; it is faſtened to the *ſpinæ apophyſes* of the *vertebræ* towards the tail, and from ſpace to ſpace adheres likewiſe to the membranes of the muſcles, which give it motion.

The ſame plan of fibres, being arrived at the firſt *vertebræ* of the back, divides firſt into two parts, which form ſeveral heads, and by different origins are inſerted into different parts; there is one about 2 inches broad, which riſes to the third *vertebræ* of the neck, and is faſtened upon the *rhomboides*; another fixes itſelf upon the head of the *omoplate*; the third on the hind and inner part of the arm, the elbow, and the upper and hind part of the firſt joint of the arm; laſtly, the fourth makes one and the ſame tendon with that of the firſt and broadeſt, and of this is formed a fifth, which is inſerted into the middle and lower part of the firſt joint of the arm.

There is nothing particular in the muſcles of the belly, except that the little oblique muſcle and the *tranſverſalis* are inſeparable.

The caſtor's liver is of a ruddy brown colour, and divided into 7 lobes, which poſſeſs the 2 *hypochondria*, and thus cover the ſtomach on all ſides. The gall-bladder is faſtened to the largeſt of theſe lobes, and uſually diſcharges itſelf into the *duodenum*, tho' M. *Sarraſin* has met with one which emptied itſelf into the *jejunum*.

The ſpleen is round and not above 4 lines in diameter, to 3 inches long; it is more firm than that of other animals, and is faſtened by 5 or 6 very ſhort veſicles to the fund of the ſtomach, as alſo by certain membranes to the kidnies, *pancreas*, and *colon*. A few conglobate glands about the bigneſs of bees are alſo found towards that end of it next the ſtomach, which is ſomewhat bigger than the other.

The kidnies are ½ an inch thick, 2 inches long, and about as many broad; the renal glands are under ½ an inch in length.

The *pancreas* is at leaſt 2 foot long, and forms an angle, the point whereof is faſtened to the large lobe of the liver, by a ſort of little threads; it is divided into two parts, one whereof paſſes under the ſtomach, and is faſtened to the ſpleen and the left kidney; the other deſcending along the *duodenum* and *jejunum*, open into the ſame by ſeveral little paſſages.

The *oeſophagus* is wholly inveſted with a white membrane, which appears like a kind of lining, and may eaſily be parted from the canal without tearing it.

The ſtomach is one of the moſt ſingular parts in this animal; it is upwards of a foot long, and 4 inches broad towards the ſpleen, but it leſſens by degrees, ſo that after about two thirds, contracts a full ½ by a projecture upwards of an inch, which advances into its cavity; after this it widens about 3 inches towards the *pylorus*, which is conſiderably prominent, and advances towards the ſpleen by a membrane faſtened to the *oeſophagus* at its other end; the widening above-mentioned ſeems to make a ſecond ſtomach, tho' its real uſe be no other than to retain the food a longer time, and eſpecially the more ſolid kinds, as wood, which here only yields a very ſlight extract, for it paſſes almoſt as it is ſwallowed down, whereas herbs, fruits, and roots, undergo an entire diſſolution.

The membranes of the ſtomach are ſo thin, that the part is apt to tear upon the leaſt ſwelling, tho' the fleſhy membrane thickens on the ſide of the *pyloras*, and fortifies it. We meet with no glands diſperſed in this ſtomach, but in lieu thereof it is beſet with about 100 veſicles or little bladders, 2 or 3 lines long, which are faſtened on the nervous membrane, and cover'd by the fleſhy one, their ſituation being between the ſtrait part of the veſicle and the *oeſophagus*; all theſe veſicles makes a kind of hemiſpherical body, 7 or 8 lines high, and about 3 inches broad at bottom; the inſide of each veſicle ſeems glandulous, but they are ſo delicate that they burſt upon the leaſt preſſing them, tho' each of them have its reſpective outlet, yet they correſpond to 12 little orifices about 2 lines broad, and ranged in 4 columns, which open into the ſtomach. When the animal is dead, theſe veſicles are found full of a white inodorous matter, of the conſiſtence of pap, but there is a deal of probability that it is fluid while the animal is alive; this matter is doubtleſs the diſſolvent of the food, which in the cold countries, and during the winter time, is only wood of the alder, elm, lime, aſh, and ſome kinds of poplar; during the ſummer they eat all kinds of herbs, fruits, and roots, eſpecially thoſe of diverſe kinds of water-lillies.

The inteſtines of this animal are very ſlender, and about 20 feet long. The *cæcum* is in figure of a ſcythe, being retained in that poſition by 2 ligaments which run, the one along its concave, and the other along its convex ſide, it is 18 inches long meaſured on the former ſide, and 30 on the latter. Its breadth is 4 inches at its large end, and may contain 5 or 6 *lb.* of water; the *colon* is 4 feet long, and the *rectum* about 15 inches.

The bladder is like that of dogs; and if the opening of the animal be continued to the root of the tail, we eaſily diſcern its teſticles, and the bundle mentioned above in the deſcription of the cutaneous muſcle. This bundle is a hollow muſcle which incloſes the yard and teſticles.

The teſticles are ſituate in the groin, reſting by their baſe on the lateral parts of the *os pubis*, and engaged in the fat. They are incloſed in ſeveral membranes furniſhed them by the *peritoneum* and muſcles of the *abdomen*, eſpecially by the *cremaſter* muſcles, whoſe fibres being circular, give them the figure of a cone: they are in all reſpects like thoſe of dogs, when they are unravelled.

The *vaſa deferentia* grow conſiderably bigger behind the neck of the bladder; but they leſſen again ere they enter the *urethra*, where they have outlets ſeparate from each other.

The *veſiculæ ſeminales* are ſo engaged under the *os pubis*, that there is no ſeeing them without ſeparating them. They are uſually two inches long and one broad toward the middle, but pointed at the two ends: their paſſages alſo open ſeparately into the *urethra*, and terminate like thoſe of the *vaſa deferentia*, in a fleſhy eminence, about the ſize of a pea, being a ſpecies of *veru montanum*; aſide of this eminence we find ſeveral little orifices of the excretory ducts of certain glands, ſituate around the neck of the bladder, which do the office of proſtrates, and are full of a white oily liquor.

The hollow muſcle is ſituate between the *os pubis* and the aperture, at which the excrements are voided; reſembling, in ſome meaſure, an ancient kind of pouch, wide and round at bottom, and narrow towards the top. A tendinous body, an inch broad, ties it faſt to the lower and middle

lip

lip of the *os pubis*, from whence it deſcends continually winding to the common orifice abovementioned.

Upon opening this kind of pouch, from top to bottom, about the middle thereof we diſcover the yard from its root to the *glans*. It divides this cavity into two,after which the hollow muſcle, folding itſelf after a certain manner, forms two other cavities, ſituate under the firſt aſide of the *glans*. In theſe four cavities are lodged the bags which contain the *caſtoreum*; tho', before we proceed farther, it may be neceſſary to ſpeak of the common orifice. This is a cavity, about 2 inches every way, when well dilated, in which terminate the bags of the *caſtoreum*, *urethra*, *anus*,and the *vagina* in females; it is about 3 inches diſtant from the root of the tail, and 4 from the *os pubis*, being blackiſh, and bordered with a very fine hair, nothing like that of the reſt of the body.

The root of the yard is faſtened to the lower lip of the *os pubis*: hence it pierces the membrane of the *anus*, in the part where the upper bags communicate. This membrane adheres circularly to the inſertion of the *glans*, as the *diaphragm* is to the *æſophagus*. The lower part of the yard, which is about 2 inches and a half long, is contained in the upper cavity of the hollow muſcle, being the part where it ſeparates into two; ſo that the *glans*, which is near an inch and half long, is intirely in the *anus*, between the outlets of the upper and lower bags. The caſtor attacks the female on the foreſide, both on account of the ſituation of the common orifice, and by reaſon of the length and inflexibility of its tail.

The *glans*, which is perfectly like that of dogs, is covered with a skin grained like ſhagreen; in the body of the yard we find a pyramidal bone, whoſe baſe is faſtened to the *corpus cavernoſum*, and is about 15 lines long.

Under the root of the yard are two bodies, about the ſize of wallnuts, faſtened to the *corpus cavernoſum*. Theſe two bodies conſiſt of ſlender *veſiculæ*, which ſwell in the time of copulation, by means of ſeveral blood veſſels which form a kind of *capſula* to the *urethra*.

In the ſame place, we find two oval glands, about 10 lines long, and 3 or four thick; their excretory veſſels, which are about the thickneſs of a common bodkin, and upwards of an inch long, open into the *urethra*; about an inch deep in the yard, the ſubſtance of theſe glands are very firm, and contains a browniſh oily liquor, which perhaps may ſerve to defend the cavity of the *urethra* from the acrimony of the urine. The like are found in rats, excepting that they are round.

The genital parts of the female caſtor are like thoſe of the female hare, coney, or rat. The *vagina* of that of the caſtor is 5 inches long, and, like the *urethra*, is not incloſed in the upper cavity of the hollow muſcle as the yard of the male is, but has its opening in the *anus*.

'Tis affirmed, that the females go 4 months with young, and bring 5, 6, or 8 at a time; yet M. *Sarraſin* has never found more than 4 in any of thoſe opened by him.

The female caſtor has 4 breaſts, two ſituate on the great pectoral muſcle, like thoſe of women, between the ſecond and third of the true ribs, and two more on the neck, about four fingers higher than the former.

The ancients, who did not diſſect with much care, could not perceive the caſtor's teſticles, as being very ſmall and ſituate in the groin. The ſize, figure, and ſituation of the bags impoſed upon them, and the firſt full diſcovery of theſe parts is owing to the royal academy.

The bags, or purſes, contained in the upper cavities of the hollow muſcle, and which we ſhall hereafter call upper bags, contain a reſinous matter; but thoſe in the lower cavities, which, for that reaſon, we ſhall call lower bags, are gathered in cluſters, incloſed under a common membrane, and full of an oily matter. The upper are double, and reſemble a wallet; each bag whereof being about 3 inches deep, and one and half wide at bottom, is placed, one on the right and the other on the left ſide of the yard. The bags deſcribe a kind of ſemi-circle, as they approach the yard, and contract by little and little to the very apertures, which are about an inch in diameter, and look into the *cloaca*.

There are 3 membranes obſervable in the texture of theſe purſes: the firſt ſimple, but very ſtrong; the ſecond much thicker, and beſet with numerous veſſels; the third is peculiar to the caſtor, being as dry as parchment, and about the ſame thickneſs, and tearing like it; but ſo folded on itſelf, that when opened, it extends to three times the ſpace as before. It is very ſleek on the outſide, of a pearl colour, and frequently ſpeckled with brown, ſometimes with red: it is uneven on the inſide, being lined with little threads, to which the reſinous matter ſtrongly adheres.

The firſt membrane ſeems only intended to contain the purſes within their juſt magnitude: the veſſels which beſet the ſecond ſupply the reſinous matter which is mixed with the blood. This membrane follows all the folds of the third, as the pia mater enters all the ſinuſſes of the brain; for the third, there would be a great deal of probability of its ſerving to filtre the reſinous mat-matter, if any glands could be diſcovered in it; we are to ſuppoſe them however very ſmall, and perhaps the threads above-mentioned may be their excretory ducts.

This matter thus filtrated thickens gradually in the purſes, till it arrive at the conſiſtence of a reſin heated between the fingers, when it is called *caſtoreum*; it preſerves its ſoftneſs above a month, after its being taken from the animal, and ſmells very rank all this time, being browniſh on the outſide, and yellowiſh within; but it afterwards looſes its ſmell, and turns hard and friable, like other reſins. It may be added, that it is combuſtible at all times; the largeſt purſes weigh about two ounces.

The lower purſes, at firſt ſight, ſeem double; one on the right, and the other on the left ſide of the *cloaca*; but upon taking off the membrane which covers them, we ſometimes find two or three together, each cluſter of theſe purſes is 2 inches ½ long, and 14 or 15 lines in diameter. The purſes are round at bottom, and leſſen inſenſibly as they approach the *cloaca*. The largeſt poſſeſſes the whole length of the cluſter, but is only 8 or 10 lines in diameter. The ſecond is not uſually half ſo big as the firſt; and the third is commonly the ſmalleſt.

Theſe purſes, beſide their common membrane, have each of them three proper ones: the firſt, being of a very delicate texture, is interſperſed with abundance of veſicles. The ſecond is not only thicker, but is likewiſe inveſted, and, as it were, cruſted over with glands, which ſeem to be conglomerate, and are ſpread in cluſters of different magnitudes over the whole external ſurface of the membrane. In the middle of theſe cluſters, we find certain cavities, which open into each other, *viz.* the larger into the ſmaller, and thoſe at length into the purſe itſelf by apertures, a line or two in diameter.

The third membrane is white, and ſo very delicate, that it tears as if it were only a cream condenſed on the inner ſurface of the ſecond. It is perforated in the ſame places as that other, in order to give paſſage to the liquor filtrated in the glands.

The firſt membrane ſuſtains the blood veſſels, which furniſh the liquor to be ſecreted by the ſecond and third. This ſecretion is performed by the glands, upon pricking them very ſlightly, yield an oily liquor, and even that in the purſe readily empties itſelf at ſuch punctures, upon ſlightly ſqueezing the purſe. This liquor is of a pale yellow colour, full of little bodies, like thoſe obſervable in oil of olives when it begins to fix; that of the caſtor afterwards becomes perfectly liquid, and of an amber colour.

The induſtry of nature is here truly wonderful, who, to prevent the little paſſages of the purſes into the *cloaca* from being ſtopped, by the thickening of the liquor, or from ſhrivelling by the action of the air, has beſet them all with a kind of hair about half an inch long, which ariſing from the purſe itſelf a little beyond the paſſage, runs the whole length thereof, and advances a little into the *cloaca*.

All theſe purſes, upper and lower, do not communicate with each other, their ducts, as already obſerved, terminate in the *cloaca*; nor do we know what uſe theſe liquors are of to the caſtor. There is no truth in what is commonly alledged, that they ſerve to excite its appetite when languiſhing; and M. *Sarraſin*, tho' he kept one of theſe animals 2 years, yet could not penetrate into their uſe. It is a miſtake likewiſe, that the hunters make uſe thereof, as a bait for catching caſtors in ſnares; but 'tis cuſtomary to anoint the ſnares laid for predatious animals therewith, as foxes, bears, martins, but eſpecially carcajoux, which laſt, in the winter time, attack the caſtors in their dens, which they frequently break up.

Among the ſavages the women greaſe their hair with the oil of the caſtor's bags, but it ſmells ill, and can have no charms, except for a ſavage.

From the *abdomen* we muſt paſs to the *thorax* of the caſtor; this part is about 5 inches long, very narrow at top, much wider towards the bottom, and cloſe by 14 ribs, *viz.* ſeven true ones, which are very ſhort, and ſeven ſpurious ones, which are not only much broader, but leave a conſiderable ſpace between them. 'Tis by this means the caſtor is enabled to contract itſelf ſo eaſily, ſince the ribs are quickly drawn together by the action of the circular fibres of the firſt muſcle.

The *ſternum* conſiſts of 5 narrow bones; the ſcutiform cartilage, which is about an inch broad, is round and very flexible; the lungs have 6 lobes, 3 on the right ſide, two on the left, and a ſmall one incloſed in the *mediaſtinum*; the annular cartilages of the *trachea* conſiſt only of one piece.

The heart is about 2 inches long, and the baſe thereof ſomewhat above an inch and ½ in diameter; its ventricles are pretty equal, but the right auricle is much ſmaller than the left, and yet I do not hereupon conclude, that the quantity of blood, falling into this ventricle, is leſs proportionably to its bulk, than what is received by the other; for the lower *vena cava* is here conſiderably widened, and forms a kind of bag, ſurrounded with fleſhy fibres, being about an inch and ½ in diameter; this bag acts in concert with the right auricle for filling the right ventricle, and being narrower next the liver, is cloſed by 3 valves like the *ſigmoides*, which permit the blood to purſue its courſe, but hinder its reflux, which otherwiſe there would be danger of, by reaſon the upper *vena cava*, in lieu of opening into the auricle, goes backwards, and empties itſelf into this bag, ſo that the two ſtreams of blood here meet in oppoſite directions; and the left ſubclavian, in lieu of purſuing its courſe into the upper *vena cava*, deſcends over the lower branch of the *aorta* under the baſe of the heart, and opens into the *ſacculus* abovementioned.

As to the head, the moſt remarkable things therein are, 1ſt, the occipital bone, which is placed like a plate on the back of the head; 2dly, there is no inner ſinus in the *falx* of the *dura-mater*, which membrane makes a ſlight diviſion of the brain, being ſuſtained by oſſicles, inſerted in its own ſubſtance, ſome whereof are only bony *laminæ*, very ſolid tho' thin, while others are round, about a line in diameter, and 2 or 3 lines long; 3dly, the brain has no ſenſible ſinus's, but the *pia mater* ſeparates from it, as if it were only laid on a ſmooth body; 4thly, the *cerebellum* has ſeveral bunches or prominences of different figures, which are ſeparated from each other by the *pia mater*; two of theſe ſpring from the ſides, and are about ⅓ of an inch every way; 5thly, the eyes are very ſmall, the aperture of the eyelids not being above a third of an inch; the *cornea* is round, and the *iris* of a deep blue; 6thly, there is a third ſort of eye-brow, ſituate in the large *canthus* of the eye, which ſerves as a curtain to cover or uncover the *cornea*, at the diſcretion of the animal; 7thly, the two jaws, which are very ſtrong and nearly equal, are each furniſhed with 10 teeth, 2 cutters, and 8 grinders; the cutters are ſituate at the end of the ſnout; thoſe on the upper ſide are about 8 lines long, and thoſe on the under ſide a whole inch; the roots or twangs of the former are 2 inches ½ long, and thoſe of the latter three, following with all the curvity of the jaws, which gives them an incredible ſtrength; accordingly the caſtor will fell the largeſt trees by meer dint of teeth; 8thly, as theſe animals live chiefly on dry foods, nature has furniſhed them with ſalival glands of prodigious bulk, to furniſh moiſtening enough; they poſſeſs all the under part of the lower jaw, the fore-part of the neck, and deſcend as low as the clavicles; they are covered with a muſcle, which adheres to the ſkin, and conſiſts of 2 plans of fleſhy fibres, faſtened to the ſecond, third, and fourth *vertebræ* of the neck, by a fleſhy origin 4 fingers broad; theſe 2 plans, taking contrary courſes, inveſt the neck towards the *trachea*, whereon their fibres croſs each other, ſomewhat in manner of a mat; that ariſing from the right ſide, proceeds towards the left, and is inſerted by its *aponeuroſis* into the arm, the bend of the elbow, and the firſt joint. The other plan, taking an oppoſite courſe, is inſerted after the ſame manner into the other arm; this muſcle is faſtened a-top to the whole lower jaw, and at bottom is ſuſtained on the fat, and deſcends to the clavicles; its uſe is to ſqueeze the glands, by forming the jaw, and bringing the arms of the animal nearer, at the ſame time that it holds its food in its hands.

The caſtor's tail has no relation to the reſt of its body; it ſeems rather to approach the nature of a fiſh, being covered over with a ſcaly ſkin, under which we find a firm kind of fat, much like the fleſh of a porpoiſe, which was probably what chiefly contributed to the caſtor's paſſing for an amphibious animal. The ſcales are hexagonal, ½ a line thick, and 3 or 4 long, being joined together by a thin pellicle, and ſet in the ſkin, from which they eaſily ſeparate, after the animal is dead. Between each ſcale ariſe three or four hairs, about 2 lines long, which are more numerous on the ſides of the tail than elſewhere.

For the motion of this tail there is a great number of muſcles, ſome large and others ſmaller; the largeſt are faſtened on the tranſverſe *apophyſes* of the *os ſacrum*, and their tendons diſtributed in bundles of 4 or 6, each encloſed in ſheaths, which carry them along the *vertebræ* of the tail. The tendons of the leſſer muſcles are faſtened and confounded along with thoſe of the firſt.

The caſtor being deſtined for works of architecture, fells and cuts wood with its teeth, ſoftens and kneads the clay with its feet, and its tail not only ſerves for a trowel, but as a hod for carrying mortar; and hence the reaſon of its being ſcaly furniſhed with fat, and with a multitude of muſcles.

The fore-feet are like thoſe of other animals, which hold their meat between their feet, and thus bite it by degrees, as rats, ſquirrels, *&c.* but the hind feet have no relation thereto, being liker thoſe of river fowls, and which are webb-footed, as ducks, geeſe, *&c.* ſo that the caſtor is fit either to walk on the ground, or ſwim in the water. From the tip of its noſe to the thighs, it reſembles a rat; but from the thighs to the tail, it is more like one of the flat-footed kind of river fowl.

To the anatomy of the caſtor, M. *Sarraſin* has added ſeveral obſervations relating to their manner of living.

Firſt, when the large floods are over, the females return to their cells to bring forth their young; the males keep the field till *June* and *July*, nor come home till the waters be quite low; they then betake themſelves to repair the damages which the floods have made in their houſes, or elſe make new ones; they change their place chiefly on three accounts, 1ſt, when they have ſpent all the food within their reach; 2dly, when the company is too numerous, and, 3dly, when the hunters moleſt them too much.

Secondly, To fix their dwelling, they chuſe a place fruitful in proviſions, watered with a little river, and fit for making a lake therein; they begin with building a cauſway of a ſufficient height, for raiſing the water to the firſt bed or apartment of their cells. If the country be flat and the river deep, the cauſways are built long, but not ſo high as in vallies; theſe cauſways are 10 or 12 feet thick at their foundation, but diminiſh as they riſe, and a-top are rarely above two. The animal having a great talent at felling wood, does not ſpare it, but cuts it uſually in pieces about the thickneſs of the arm or thigh, and from two feet to ſix feet long; theſe they uſe as ſtakes, ſinking one end of them deep in the ground, and very cloſe to each other, waitling them with other ſmaller and more pliable pieces, and at length filling up the vacuities with clay. This work they continue while the water riſes for the greater conveviency of tranſporting their materials, but at length they put a ſtop to this kind of dyke; when the waters retained may reach the firſt ſtratum of the habitation intended. That ſide of the cauſway, contiguous to the water, is built ſloping, ſo that the water acting in the direction of its depth, preſſes it ſtrongly againſt the ground; at the ſame time the oppoſite ſide is perfectly perpendicular; this work is ſtrong enough to bear people who get upon it, and the animals take great care to keep it in order, never failing to repair the ſmalleſt breach with clay; if they perceive that the hunters obſerve them, they only work in the night-time, or elſe quite abandon their dwelling.

Thirdly, when the cauſway is finiſhed, they go to work on their cabbins, which they always build very ſtrongly at the edge of the water, on ſome little iſland, or in piles drove for the purpoſe. Theſe lodges are either round or oval, and ſtand about $\frac{2}{3}$ out of the water, but they take care to leave a door, which the ice cannot ſtop; ſometimes they build the whole houſe on dry ground, and dig ditches five or ſix feet deep, to the edge of the water. The ſame materials are uſed for their houſes as the cauſways, tho' the former are perpendicular, and terminate in a kind of dome, the walls being uſually two feet thick; as their teeth are equivalent to the beſt ſaws, they cut off all the ends of timber which come beyond the walls, and plaiſter them over both inſide and out, with a kind of mud made of clay, and dried herbs, and here their tail ſerves them in good ſtead to beat and lay this plaiſter the cloſer.

Fourthly, the inſide of the cell is roofed with a flat vault, and may ſerve to lodge 8 or 10 caſtors; on the out-ſide it may meaſure 8 or 10 feet broad, and 10 or 12 feet long, ſuppoſing it oval; within-ſide it may be 4 or 5 feet broad, and 5 or 6 long, if the number of caſtors to inhabit it be 15, 20, or even 30, which however rarely happens. The houſe is made large in proportion, or even ſeveral houſes are built cloſe to each other. Some miſſionaries aſſured M. *Sarraſin*, that they had found four hundred caſtors lodged in different cells, which communicated with each other; they are built in ſtories, that the animal may have a retreat when the waters ſwell; they have alſo an aperture beſide the door, and the place where they bathe; by this aperture they go to the water, to void their excrements.

Fifthly, the name terrier caſtors is given to ſuch as lodge in caves, built where the ground riſes over the edge of the water; theſe begin their houſe by an aperture, which goes farther or leſs into the water, as the ice may be more or leſs thick, and continue it 5 or 6 feet long, but make it no broader than is neceſſary to afford them paſſage; this done, they make a lake 3 or 4 feet every way, wherein they bathe themſelves at pleaſure; then they dig another gut in the ground, riſing in ſtories one over another, that they may always place themſelves dry when the waters ſwell; theſe guts are ſometimes a hundred feet long; the caſtor covers the place where it lies with graſs, and in winter makes a kind of ſhavings, which ſerves it for a quilt.

Sixthly, All theſe preparations, eſpecially thoſe of the caſtors, which inhabit cold countries, are uſually finiſhed in *Auguſt* and *September*, which is the time when they are to begin making proviſion for the winter's ſubſiſtence; they cut wood therefore in pieces, from 2 or 3 feet long, to 8 or 10, the largeſt whereof are drawn by ſeveral of theſe animals together, and the ſmaller by a ſingle one, but all in different roads, to prevent their obſtructing one another. They firſt lay a certain quantity of theſe to float in the water, then lay new ones on the firſt, and continue thus heaping one piece over another, till their ſtore be anſwerable to the number of animals which intend to live together. Thus the proviſion for 8 or 10 caſtors is 25 or 30 feet ſquare, over 8 or 10 feet deep. This wood is not laid like that in our yards, but is ſo diſpoſed, that they may pull out what piece they pleaſe, for they only eat thoſe which dip in the water; before they eat them they chop them ſmall, and bring them into that part of the cell where they lie; if they had chopped ere they laid them up in their wood-yard, the water would have diſperſed and driven them aſunder.

Seventhly, As to the caſtor-hunting, it is performed from the beginning of *November* to *March* and *April*, they being then well furniſhed with hair. The way of taking them, is either by lying cloſe and ſhooting them, or laying ſnares, or making holes for them in the ice: the firſt is the moſt tedious, as well as the moſt precarious way; the ſecond is the moſt common, tho' the caſtors have ſufficient proviſions, yet they will be going from time to time into the woods to ſeek for new food, and the hunters, who know that they love freſh wood better than that which has been long kept, bring it them near their houſes, and lay traps for them, like thoſe wherewith we catch rats, ſeveral ſtakes 3 or 4 feet long, being driven deep into the ground, there is one heavy croſs ſtake raiſed a foot and a half high, under which, by way of bait, is placed a branch of poplar, 5 or 6 feet long, which leads to another very ſlender branch. This laſt correſponds ſo nicely to the croſs piece, that the caſtor may ſhake the firſt as long as it pleaſes, the croſs piece never falls till it begins to knaw the little branch, and then it is infallibly a priſoner.

Eightly, The laſt method is to dig holes in the ice, when not above a foot thick, the caſtors will not fail coming to theſe holes to breath, and here the hunters are ready to attack them with axes. Some hunters will fill up theſe holes with flocks of typha, to prevent their being ſpied by the animals, and in this caſe catch them by a hind foot. If there be any rivulet near the cabbin, they cut the ice a-croſs, and lay a ſtrong net therein, which done, going to ſtorm the cabbin and drive out its inhabitants, they naturally run for ſafety into the rivulet, and throw themſelves into the ſnare.

VII. *The deſcription aud uſe of a level of a new conſtruction, by M.* de la Hire *.

The great aqueducts, which the ancients have made, might have perſuaded us, that they were very ſkilful in the art of levelling, if the inſtruments which they uſed, and the whole artifice, which they employed, had not been handed down to us in the works of *Vitruvius*. Their great level, which they called *chorobates*, was a piece of wood 20 feet long, ſupported by ſome pieces at the extremities, and which had in its upper part a canal that they filled with water, with ſome little plummets, which hung to the ſides, to be aſſured if the piece was in a level; and this was the whole extent of their levelling, for they carried the *chorobates* from 20 feet, to 20 feet to conduct their work.

* Nov. 12, 1704.

But the new diſcoveries that have been made in the academy of ſciences, and the different levels that have been there invented with teleſcopes, which ſerve them for ſights, have given us the means of making levels with a great deal more juſtneſs, and with much more eaſe, than all thoſe which have been made till this time, ſince we may level at once a diſtance of 1000 toiſes, without any ſenſible error.

We have in our hands the levels of M. *Picard*, *Mariotte*, *Huygens*, *Thevenot*, and *Roemer*, whoſe conſtructions are all different, and I have alſo given one which draws its juſtneſs from the ſurface of water; a deſcription of which I have printed in M. *Picard*'s treatiſe of levelling in 1684, and which was afterwards copied in the memoirs of the academy in 1699. But the ſurveys that I have formerly made by order of the king, in different places and at different times, for the ſpace of about 100 leagues, have ſhewn me, that almoſt all theſe levels have great inconveniences in them, and that they cannot eaſily be tranſported without our being obliged to rectify them, and ſometimes to reſtore the ſights, which require pretty troubleſome operations. Not that it is neceſſary to make uſe of a juſt level to make exact ſurveys, ſince we may eaſily arrive at it by taking ſome precautions, as is obſerved in M. *Picard*'s levelling, and as I have explained it afterwards in a little treatiſe of levelling, which I have given to the publick.

The * level, which I here deſcribe, and which I made ſometime ago, is conſtructed upon a different principle from all other levels which have hitherto appeared: for they draw all their juſtneſs, either from the centre of gravity of the body of the whole level, which is ſuſpended upon a body flexible or otherwiſe, or from a plummet alſo ſuſpended to a thread, as fine as a hair; or laſtly, from the ſurface of water, or ſome other fluid body, whoſe ſurface is always level. But this here is not at all ſuſpended by any body whatever; on the contrary, the centre of gravity of the body which ſerves it for a rule, as it does to many others, is placed above the point of ſupport; ſo that if it was poſſible, that the centre of gravity of the body, which is a mathematical point, ſhould remain immoveable upon the ſupport, which is the niceſt point that it is poſſible to make; we ſhould then have the true level, which would be the perpendicular line drawn to that which paſſes through the centre of gravity and through the ſupport. But as it is impoſſible to make the centre of gravity ſtay in a fixed place above the point of ſupport, we take for the true level the poſition where this point is indifferent to fall either on one ſide or the other.

---

* Plate IV. Fig. 19.

It will perhaps be ſaid, that this principle is not ſo juſt as that, where the heavy body is ſuſpended by a flexible body; but if we make the leaſt reflection upon theſe ſuſpenſions, we ſhall ſee that there are very great irregularities cauſed by the nature of theſe bodies; beſides, if the whole machine is expoſed to a little wind, it is in a continual ſhaking and agitation, and it is impoſſible to determine the level. I ſay nothing of the remedies, that are brought to theſe ſhakings, as of plunging the weight ſuſpended to the machine in water or oil to ſtop the vibrations, ſince even this remedy has great inconveniences, and yet is not ſufficient.

This which I propoſe is firm, ſolid and ſteady, being once well conſtructed, and it may be carried about as we pleaſe, without being afraid that it may alter; and if it makes any alteration, it will be very eaſy to rectify it without changing its place, or, as we ſay, a ſingle ſtation.

The new ſights upon glaſs, as I have propoſed them to the academy, and as they are deſcribed in the memoirs of 1700, and in my aſtronomical tables, give it alſo a very great advantage above all the reſt; for theſe ſights are unalterable by all the changes of the air, and cannot be ſpoiled by any other accident whatever, tho' they ſhould be at leaſt as fine and ſlender, as the threads of a ſilk-worm, which have been hitherto conſidered as the moſt fit to make exact obſervations with. It is very eaſy to make them, ſince it is only a little line, or very fine ſtroke, marked with a diamond upon a little bit of glaſs, which is fixed to the *focus* of the object-glaſs of the teleſcope; for the centre of the object-glaſs is in the room of an object-ſight, and the ſtroke upon the glaſs ſerves for an eye-ſight. It is eaſy to judge, that this eye-ſight cannot ſuffer any alteration or change, either by heat or cold, by little inſects which fix themſelves to the ſilk, by touching it, or, in fine, by carrying the inſtrument in any manner whatever; for it will be as ſolid as the object-ſight.

The principal part of this level is an iron rule, which bears at its extremities the ſights or diopters, which are an object-glaſs of a teleſcope for the object-ſight; and for the eye-ſight, a little bit of glaſs with a little ſtroke, as I have juſt ſaid. This glaſs runs in a frame or groove, that it may be ſtopped at what height we pleaſe. This rule is inſtead of the body of a teleſcope, which has no eye-glaſs, and has no other tube than the whole box, which is blacked on the inſide and well cloſed.

There is under this iron rule another rule placed upon the ground, which ſerves to make it more firm, and has in the middle another piece of iron or braſs, about $\frac{2}{3}$ of the length of the firſt, with which it is placed in a ſquare. This piece increaſes in width at the bottom, ſo that the breadth is perpendicular to the length of the rule of the teleſcope. At the extremity of this piece are ſoldered on both ſides the pivots, upon which the machine moves; and conſequently as the whole body of the level is moved when the pivots are pretty near upon a level, the teleſcope riſes and falls in its motion.

There is a little care required in the conſtruction of the pivots. Their cut muſt be in form of a very acute lozenge, with a ſharp point, both at top and bottom; for it is by theſe places that the whole level is ſuſtained upon the ſupport.

The pivots are of tempered ſteel, and the place where they bear upon the ſupport is a little hollow, altho' ſharp, that in the motion of the level, they may not get from either ſide. There are two ſimilar cavities oppoſite to one another for an uſe which we ſhall hereafter explain.

The ſupports are faſtened with ſcrews to a particular piece, which is of wood, and this piece is held in the bottom of the box with ſcrews, in ſuch a manner, that it may be drawn out of the box with the whole level which reſts upon its ſupports.

Theſe ſupports are made of a flat piece of tempered ſteel, in which are two round apertures, of which the inner one is blunted, to ſuſtain the pivots of the level, and to be firm enough not to ſpoil, in the motion of the level.

The box that incloſes the level, may be made of what figure you pleaſe on the outſide; But the whole level being of the figure of a T, the box ought to be pretty near the ſame figure, and it muſt hold the inſtruments in ſuch a manner, that it has only liberty to move a little on each ſide. At the two extremities of the upper part of the box, which is the tranſverſe of the T, there are two round covers, to one of which is fixed the end of the tube, where the *porte oculaire* enters; for the eye-glaſs is not faſtened to the teleſcope, and we may by this means bring it nigher or farther from the eye-ſight, according to the ſtrength of the obſerver's ſight. The aperture of the other end of the box ſerves to let the rays of the objects paſs, which meet the object-glaſs, or object-ſight.

Toward the eye-glaſs at the lower part of the length of the box, is a pretty long ſcrew, which moves in a nut fixed to the box. This ſcrew ſerves to raiſe the end of the iron rule, as occaſion requires. The furrows of this ſcrew are very fine to raiſe the rule as it were inſenſibly by turning the ſcrew.

Oppoſite to the ſcrew, that is, in the top of the croſs-way of the box, there is within a very ſlender ſpring, which is held to the box by the extremity fartheſt from the eye-glaſs, and at the other it bears a peg with a button, which paſſes to the out-ſide of the box. This peg ſerves to drive the ſpring downwards; for in its natural ſtate it is applied againſt the top of the box on the inſide.

There is at the ſcrew a *repaire*, or mark, to let it into the nut juſt in this place, and then the rule of the teleſcope, being reſted upon the ſcrews, the teleſcope is over-againſt the apertures of the box.

There is then nothing to be done but to place the ſights, ſo that their true line be perpendicular to the plane, which paſſes through the centre of gravity of the whole level, and through the line of the ſupports, and this is called rectifying it; and as all the parts that compoſe it are ſolids, being once well rectified, it will not afterwards change: wherefore it is ſufficient to rectify it well in conſtructing it, by ſome of the ways which are explained in M. *Picard's* treatiſe of levelling, by eſtabliſhing two points of level, and alſo with the level without being rectified.

To rectify the level.

Suppoſing then that we have two points of level A and B, about 100 toiſes diſtant from one another, we place the teleſcope at one end of theſe points as A, and point it toward the other point B, raiſing or ſinking the box gently. And when the object B appears upon the eye-ſight, if the rule of the teleſcope is placed upon the point of the ſcrew, and it is not in a ſtate of falling forward, that is, toward the object-glaſs, which we may know by puſhing or raiſing the rule a little, by turning the ſcrew; for then the teleſcope falls lower, and it is alſo placed upon the ſcrew. This ſhews, that the part of the whole level, which is towards the eye-glaſs, is too heavy; therefore the part of the rule toward the object-glaſs muſt be changed, by means of a little weight which ſhall run upon the rule, and may be ſtopped in whatſoever place we pleaſe with a little ſcrew, or only charging it with a little ſoft wax and lead. We charge this part of the rule ſo much, that the ſights being pointed toward the object B, are indifferent to reſt upon the point of the ſcrew, or to fall forward, and then it will be rectified.

But if at firſt the ſights being pointed toward the object B, the rule cannot keep upon the ſcrew; we ſhall know by that the part of the level toward the eye-glaſs is too light, and then it muſt be charged as was done in the other caſe, till the level is in an indifferent ſtate of falling or reſting upon the point of the ſcrew; and then the level wiil be rectified.

We might alſo, inſtead of charging the rule on either ſide, raiſe or ſink the eye-ſight in its frame, till the level was juſt; which we might do if the level is not far from being juſt.

We ſhall obſerve, that in theſe operations every time that the teleſcope falls forwards, it is replaced upon the point of the ſcrew, by puſhing the ſpring at the top of the box, which drives the frame of the eye-ſight downwards, and by that means replace the rule upon the point of the ſcrew.

As this level may be put in a reverſe ſituation, which is when the teleſcope is below the pivots, we muſt explain what ought to be obſerved in the conſtructing of it to ſerve in this ſtate.

It is certain, that if the pivots were a mathematical line, when the level is reverſed, it would point to the ſame place where it pointed in its right ſituation, provided that this place was perfectly level with the place where the level is ſituated; and to rectify it, we need only aim the teleſcope at the ſame point in the two ſituations of the level, by increaſing or diminiſhing by degrees the weight of one of the ends of the rule. But as the pivots muſt have a conſiderable thickneſs to make them ſolid, it may happen that the plane, which paſſes thro' the centre of gravity of the whole level, and through the line where the pivots reſt, will not be the ſame in the two ſituations of the level; it muſt be rectified in the reverſed poſition, the ſame as in the right. But as we muſt not change the ſights which are rectified, for the right poſition, we muſt correct the place of the pivots, where they bear upon the ſupport in the reverſe poſition.

The level being therefore reverſed, if the ſights do not give the ſame point, as in the right ſituation rectified, the pivots muſt be filed a little to drive the point toward the object glaſs, if the ſights fall too low in the reverſe ſituation, or toward the eye-glaſs if they riſe too high.

The pivots may alſo be made in another manner, ſo that the plane which ſhall paſs through the centre of gravity of the inſtrument, and through the place where the pivots reſt, ſhall be the ſame both in the right and reverſed ſituation of the level, and thus we may rectify this level from a ſingle ſtation, without having a line of level.

In this ſecond manner, the pivots muſt be cylindrical, and very well turned and poliſhed, in the places where they bear upon and touch their ſupports, which ought to be a little hollow like a pully. Beſides, the bottom and top of the hole of the ſupport which ſuſtains the pivots, muſt be almoſt in a right line, that the pivots may not touch ſenſibly but in one point.

It is evident, that in this conſtruction of the pivots, the centre of gravity of the level, and the line which ſhall paſs thro' the places where the pivots reſt in the two ſituations of the level, will be always in the ſame plane, which ſhall alſo paſs thro' the axis of the cylinder of the pivots.

This ſuſpenſion of the level is not ſo nice as the firſt, becauſe the place of the pivot, where it touches upon the ſupport, is of a circular figure,

gure, and in the firſt it is of a pointed figure a little blunted.

The uſe of the level.

To make uſe of this level, we muſt firſt put the ſcrew to its mark, that the ſights may be over againſt the apertures of the box. Afterwards, by the means of the ſprings, we drive the rule of the teleſcope againſt the end of the ſcrew, in ſuch a manner, that it does not fall from the other ſide, which is eaſy to do by inclining the box; and in this ſtate, the rule of the teleſcope being placed upon the ſcrew, without the ſpring holding it, we bend the box by degrees toward the end, where the object glaſs is, till the teleſcope falls on that ſide. Then we hold the box as firm as poſſible in this ſtate, either upon a foot or againſt ſome other ſolid body; the teleſcope muſt be then pointed toward the object, that we would level, and we ought to obſerve exactly the part of the object that appears upon the ſtroke, which ſerves for an eye-ſight. But as I ſuppoſe the teleſcope to be placed upon the ſcrew, we muſt puſh the ſcrew gently, till the teleſcope falls on the other ſide, and obſerve well the part of the object, which appears upon the ſtroke, when the teleſcope falls; and to be aſſured of it we muſt drive back the teleſcope upon the ſcrew by means of the ſpring, and obſerve if it falls again; and if the ſame object appears upon the ſtroke of the glaſs, which appeared there before. We ſhould then undo a very little part of the ſcrew, and driving back the teleſcope upon the ſcrew, obſerve what difference there ſhall be between the object, which ſhall appear upon the ſtroke in this ſtate of the teleſcope, and that which appeared there before. It muſt alſo be conſidered, if the teleſcope reſts then upon the ſcrew, or if it falls again from the other ſide; for to have the point of the level juſt, the teleſcope being placed upon the end of the ſcrew, muſt be in a ſtate of falling on the other ſide, without falling, and we cannot raiſe it the leaſt with the ſcrew without its falling; it is then that the object, which appears in the teleſcope upon the ſtroke of the ſight, is in a level with the ſtroke of this ſight.

What I ſay of the level, muſt always be underſtood of the apparent level, with relation to the place where the inſtrument is, which ſerves to level, as it is explained in the treatiſe of levelling; for to have the true level correſponding to that where the inſtrument is placed, we muſt make the correction to the apparent point of level, with relation to the diſtance between the place where the inſtrument is, and the point levelled; but we do not here treat of the practice of levelling.

One of the advantages of this level is, that it may be reverſed, and ſerve again to level in this ſituation, without any preparation or change; and tho' it has then ſome vibrations, they laſt but a little while; for they come only from the body of the inſtrument, and not from the motion of the box, which muſt be held firm, but without any ſubjection, ſince it loſes nothing of the juſtneſs of the level, we may alſo eaſily ſtop the ſhaking of the level, by ſupporting gently the ſpring againſt the frame of the ſight, and by letting it afterwards replace in its natural ſtate.

The ſize of this level is not at all determined; for the greater the teleſcopes are, the more diſtinct the diſtant objects will appear; but it will be much more inconvenient to be carried about.

I have proved that one of theſe levels, whoſe height and length was but 10 inches, determined the level to near 3 or 4 inches at a diſtance of 1000 toiſes, which is all we can hope from a level, ſince an object of this bigneſs, at this diſtance, is entirely covered by a ſingle thread of ſilk.

We may make uſe of different ſupports; but one of the moſt convenient ways for practice, will be to fix the box of the level very firm with a ſcrew, againſt the croſs of a painter's eaſel, which is ſolid; we might alſo hold it only in the hand, and bear it againſt ſome very ſtable body, whilſt the operations are performed.

It muſt be obſerved, that when we would tranſport this level, the pivots ſhould not bear at all upon their ſupport, which will be very eaſy, if theſe pivots jut a little out of the box, and if there is in this place upon the box two ſorts of hooks, which are engaged in the ends of the pivots, and hold all the level raiſed out of the ſupports. We might alſo fix the teleſcope againſt the end of the ſcrew, by means of the ſpring, which ſhall be reſted upon the ſight; but if we would open the lid of the box, we might engage the whole level between ſome brackets, which will hinder it from balancing on one ſide or other, in carrying it a long journey.

PAP
of
F
1

A
TABLE
OF THE
PERS contained in the ABRIDGMENT of the HISTORY and MEMOIRS of the ROYAL ACADEMY of SCIENCES at PARIS, for the Year MDCCV.

In the HISTORY.

ON *the different heights of the* Seine, *at different times.*

Of a proper veſſel in which bodies are to be expoſed to a burning mirror.

. *Of ſome extraordinary lights ſeen in the heavens.*

. *Of an unuſual ſmell in the wines of* Anjou.

Of ſome extraneous foſſils found at Anjou.

. *Of a ſort of bee-hives found in ſtones in the* Upper Egypt.

I. *Of the rolling of the drops, which fall from the alembic, in the diſtillation of ſpirit of wine.*

II. *Of the preſervation of water in long voyages.*

. *On a method of preſerving inlaid work from the worms.*

Of an exceſſive heat felt at Montpellier.

. *Of the effects of the burning mirror in that weather.*

I. *Of the yellow amber.*

II. *Of M.* De L'Iſle's *map of the* Roman *empire.*

In

In the MEMOIRS.

I. *Obſervations on the quantity of rain-water, which fell at the royal obſervatory during the year* 1704, *with the heights of the barometer and thermometer, and obſervations on the winds that reigned, by M.* de la Hire.

II. *A compariſon of the obſervations on the rain and winds made by M.* de Pont-briant, *at the caſtle of* Pont-briant, *two leagues from* St. Malo, *and towards the ſea-coaſt, during the year* 1704; *with thoſe which were made at the obſervatory at the ſame time; by M.* de la Hire.

III. *Reflections on the obſervations of the variation of the needle, made in the pope's legat's voyage to* China, *in the year* 1703; *by M.* Caſſini *the ſon.*

IV. *Of barometers without quickſilver, for the uſe of the ſea, by M.* Amontons.

V. *Reflections on the rules for the condenſation of the air, by M.* Caſſini *the ſon.*

VI. *That the experiments uſed to prove that fluids are firſt condenſed and cooled before they are dilated at the approach of heat, do not prove it, and that this apparent condenſation is merely the effect of the dilatation of the glaſs and of the veſſels which contain theſe fluids, by M.* Amontons.

VII. *Obſervations on the declination of the needle, made in a voyage from* France *to the* Eaſt-Indies, *and in the return from the* Indies *to* France, *in the years* 1703 *and* 1704, *by M.* Caſſini *the ſon.*

VIII. *New obſervations on the load-ſtone and on the magnetical needles, by M.* de la Hire *the ſon.*

IX. *On the condenſation and dilatation of the air, by M.* de la Hire *the ſon.*

X. *Experiments on the rarefaction of the air, by M.* Amontons.

XI. *On the* ecumes printanieres, *or cuckow-ſpittle, by M.* Poupart.

XII. *Philoſophical experiments on the refraction of musket-balls in water, and on the reſiſtance of this fluid, by M.* Carré.

XIII. *A compariſon between our obſervations on the barometer, and thoſe made by* F. Sebaſtien Truchet, *by M.* Maraldi.

XIV. *Remarks on ſome experiments made with ſeveral barometers, and on the light made by one of them, which was agitated vertically, by M.* de la Hire *the ſon.*

XV. *On the height of the quickſilver in the barometers, by M.* Amontons.

XVI. *A continuation of the ſame obſervations, by M.* Amontons.

XVII. *A continuation of the ſame obſervations, by M.* Amontons.

XVIII. *Experiments on the capillary tubes, by M.* Carré.

XIX. *A continuation of the obſervations on the height of the quickſilver in the barometers, by M.* Amontons.

XX. *New reflections on the rules for the condenſation of the air, by M.* Caſſini *the ſon.*

XXI. *An experiment on the quantity of heat reflected to us by the moon, by M.* de la Hire.

AN ABRIDGMENT OF THE

PHILOSOPHICAL DISCOVERIES and OBSERVATIONS in the HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1705.

I. *On the different heights of the* Seine *at different times; tranſlated by Mr.* Chambers.

EVERY thing is to be obſerved, and the preſent obſcurity of phyſicks is not more owing to this, that cauſes are hid, than that effects themſelves are yet undiſcovered. M. *Amontons* had begun to procure obſervations of the different heights of the *Seine*, at different times, by means of a friend, whoſe houſe was commodiouſly ſituate for that end, and who had taken a fixed point on the maſſive of *Pont-Neuf*, which ſupports the equeſtrian ſtatue of *Henry* IV. Hence he told day by day the riſings or fallings of the *Seine*, upon an immovable ſcale fitted thereon, which he could obſerve with a teleſcope. M. *Amontons*, having the journal of theſe obſervations, from the 14th of *September*, 1703, to the laſt of *December*, 1704, digeſted them in the following manner.

He divided the whole into riſes and falls of water, noting, 1ſt, for inſtance, how many days the water continued riſing from the beginning of the obſervations, and how much it had roſe; then how many days it continued falling, and what the whole fall was; then again, how many days it had riſen, and ſo on.

By the bare journal of theſe obſervations it appeared, at what time of the year the water had been higheſt or loweſt; how much it had been higher one year than another, *&c.* and by this diviſion of the obſervations, into riſes and falls, the number of riſes and falls each year, with their duration, magnitude, and other relations, exactly appeared.

For an inſtance, from the 14th of *September*, 1703, to the 10th of *February*, 1704, there appeared to have been 8 riſes, all which together made 223 inches, and it continued 77 days; that from the 10th of *February* 1704, to the 18th of *September* following, there had been 8 more elevations, which only made 163 inches, and it continued 70 days. Hence M. *Amontons* inferred, that the rains whereby the *Seine* is ſwelled, had been more haſty, and ſucceeded each other faſter from the autumnal equinox of 1703, to the vernal equinox of 1704, than from this laſt equinox, to the autumnal one following; inaſmuch, as the ſum of the former elevations was almoſt double that of the latter, tho' in times nearly equal.

As to the different falls of the water in the ſame times, their quantity bore a nearer relation to their duration; hence it may be inferred, that as the waters do not fall ſo readily as they riſe, 'tis probable that rivers while they are high, ſend waters into the ground, which return to them again, and ſerve to ſupport them.

Theſe notions we only give as a ſpecimen, of the conſequences that might be drawn from a ſufficient number of juſt obſervations, upon the heights of rivers at different times; they who have any reliſh for philoſophy, and have opportunities of making them, we hope will be invited hereby to put their hands to the plow.

II. *Of a proper veſſel in which bodies are to be expoſed to a burning mirror.*

The bodies which are expoſed to the burning mirror of the royal palace, can only be put into a great hollow coal, becauſe all other veſſels either melt or break with ſo great a fire. But M. *Homberg* has obſerved, that this coal muſt be of green wood, and not of dry. The latter is full of cracks, becauſe when they make it, the flame paſſes thro' the wood with too much rapidity, and in too great a quantity, and conſequently it is leſs proper to contain the bodies in fuſion, which we would preſerve.

III. *Of ſome extraordinary lights ſeen in the heavens.*

Father *Laval* a jeſuit, who is at *Marſeilles*; and Meſſ. *de Plantade* and *Clapiés*, who are at *Montpellier*, ſent to M. *Caſſini*, with ſeveral aſtronomical obſervations, the account of a luminous *phænomenon*, which had been ſeen *Dec.* 26, 1704, at $5^h\ 30'$, in the evening at *Marſeilles*, and at 5^h $\frac{1}{4}$ at *Montpellier*. We cannot doubt by the circumſtances of the two accounts, but that they were the ſame. At *Marſeilles*, where it was beſt obſerved, F. *Laval* ſaw a very luminous beam driven pretty ſlowly from E. to W. the wind was E. It came from near *Venus*, at leaſt as could be judged by the eye, and went to the ſea where it plunged, at moſt two leagues broad. There were ſeen before at *Marſeilles*, or thereabouts, 2 beams like this, and having the ſame motion. At *Montpellier* they ſaw, at the hour mentioned, a globe of fire fall at ſome diſtance from the city. The air was then very clear and calm, and a faint yellow colour tinged all the W. for more than 10 degrees in height.

IV. *Of an unuſual ſmell in the wines of* Anjou.

M. *Lemery* was told by M. *Deliſle*, apothecary at *Angers*, that the beſt wines made at *Anjou* in 1704, had, in a fortnight or month after they were made, a ſmell of burnt horn, which only increaſed with time. It always retained it very much, altho' they changed the caſks.

V. *Of ſome extraneous foſſils found at* Anjou.

The ſame M. *Deliſle* found at *Anjou*, in a ſhallow quarry, very diſtant from rivers and ponds, ſome of the pretended tongues of ſerpents petrified, ſuch as are found at *Malta*, and which are in reality the teeth of ſharks petrified.

He found alſo in a quarry, where the ſtone is very ſoft and hardens afterward in the air, an infinite number of little figures of ſhells, which in ſome places had only the firſt traces, and were a ſort of embryos; in others they were more formed, and in others perfect.

We may add to theſe obſervations, what has been ſaid upon the ſame ſubject, in the hiſt. of 1703 *.

---

* Page 13 of this volume.

VI. *Of a ſort of bee-hives, found in ſtones in the* Upper Egypt.

M. *Dodart* having received from M. *Lippi*, who travelled into *Ethiopia* with M. *du Roule*, the king's envoy, a letter dated from *Siout*, in *Upper Egypt*, *Sept.* 5, 1704, which containing a ſingular fact, he communicated it to the company. M. *Lippi* found upon the mountains of *Siout*, at the entry of a vaſt cavern, a body of real ſtone of an irregular figure, quite porous, which he had the curioſity to open. He was very much ſurpriſed to ſee it all divided into oval cells of 3 lines broad, and 4 long, placed even every way, and not communicating at all together, lined on the inſide with a very fine membrane, and, what is the moſt wonderful, each incloſing either a worm or a *chryſalis*, or a fly perfectly like a bee. The worms were very hard and ſolid, and might paſs for petrified; the *chryſalides* and flies were not, but only dried and preſerved like the ancient mummies. Many of the flies had under them little oval grains, which ſeemed to be eggs. There was at the bottom of many of the cells a thick juice, blackiſh, very hard, which appears red againſt the light, very ſweet, making the ſpittle yellow, and inflaming it like a reſin. It is in ſhort real honey. M. *Lippi* imagines that it is a natural hive, which was at firſt formed of a looſe, light, and ſandy earth, and which afterwards, by ſome particular accident was petrified. The animals which inhabited it had been ſurpriſed by the petrification, and were fixed in the ſtate in which they were then found. Their dried mucoſity had formed the membrane which lined the cells. When the hive was ſoft, the worms and flies went out to ſearch for food, and the flies made their honey in it.

Looking in the ſame place for new light into this fact, M. *Lippi* found in many places the beginnings of like hives. Theſe were as the firſt bed, formed of a number of little cells, of which the moſt part were open, and contained the animal, either in a worm, a *chryſalis*, or a fly, but dry and very hard, as were alſo the hives that were begun. He ſaw alſo upon one of theſe firſt beds, a ſecond compoſed of a heap of little boſſes, about 5 lines high, and an inch in diameter at the baſe. They were crumbling, eaſy to reduce into powder, and very like little mole hills. M. *Lippi* opened them by ſtriking them very lightly, and he always found 2 or 3 oval cells quite filled with yellow worms, which were full of juice.

It is eaſy to imagine, that upon the firſt bed once formed, there were others formed afterwards, which compoſed the whole hive; but how are theſe beds formed? From whence comes the earth of which they are made? Did the animal bring it? And how did it bring it, and in ſo great a quantity? We know no more yet. Time can only bring this knowledge.

VII. *Of the rolling of the drops, which fall from the alembic, in the diſtillation of ſpirit of wine.*

M. *Homberg* ſays, that in the diſtillation of ſpirit of wine, the drops which fall from about a foot and half high from the ſpout of the alembic on the liquor already diſtilled, roll thereon like peaſe on a table, that from the greater height they fall the better they roll. So that were they to fall only one inch, this would not happen, and alſo they roll ſo much the better the hotter they are, and that were it water inſtead of ſpirit of wine, the experiment would never ſucceed.

He ſays, that all the parts of ſulphurous liquors being impregnated with the matter of light, and their whole ſurface being ſet round with it, and this ſo much the more the hotter they are, or that by the greater length of their fall, they have collected a greater quantity of it in the air, this matter performing the ſame effect as an infinite number of little points would do proceeding outwardly, ſupport the drops of their liquors, and make them roll.

VIII. *Of the preſervation of water in long voyages.*

It having been aſked, whether, for the preſervation of water in long voyages, it could not be ſulphurated like wine, M. *Homberg* anſwered, that wine was preſerved in this manner only, becauſe the acids, which it hath naturally, being not in a ſufficient quantity in proportion to the other principles, all its principles would eaſily diſunite by the fermentation cauſed by the heat of the climates through which they paſſed, or the meer motion of the voyage; after which the wine would be no longer wine, but that the ſulphur gave it new acids, which made the doſe of this principle ſufficient; but that there could be no room for this in water, which is ſpoiled only through ſome foreign matters which are mixed with it, and ferment, or by the eggs of worms which are hatched, theſe eggs being either in the water itſelf, or in the wood of the veſſels. In this laſt caſe, a matter is requiſite, which may hinder them from hatching without ſpoiling the water.

IX. *On a method of preſerving inlaid work from the worms.*

On this occaſion M. *Homberg* added, that a perſon of quality in *Provence*, not knowing how to preſerve inlaid work from the worms deſtroying it in a few years, as it frequently happens in that country, he had adviſed him to ſteep his inlaid work in water wherein corroſive ſublimate had been mixed, which ſucceeded very well.

X. *Of an exceſſive heat felt at* Montpellier.

M. *de Plantade* wrote to M. *Caſſini* an account of the exceſſive heat which has been felt this ſummer at *Montpellier*, more eſpecially on the 30th of *July*. There was no memory of any thing that came near it. The air was that day almoſt as ſcorching as that which comes out of the furnaces of a glaſs-houſe, and there was no place to fly from it but into cellars. In many places they baked eggs in the ſun. The thermometers of M. *Hubin* broke by the liquor aſcending to the top. A thermometer of M. *Amontons*, which M. *Plantade* had, though it was in a place where the air could not freely enter, roſe very near that degree wherein tallow uſes to melt. Moſt part of the vines were ſcorched on this day alone, which had never happened in this country. The aſtronomers of *Montpellier* obſerved, that during this burning ſummer, pendulums went much too faſt.

At *Paris*, the 6th of *Auguſt* was much hotter than the 30th of *July*. A thermometer of M. *Hubin's*, which M. *Caſſini* had made uſe of 36 years, broke about 2 of the clock: it is therefore certain, that for at leaſt 36 years, it had not been ſo hot at *Paris*.

XI. *Of the effect of the burning mirror in that weather.*

Who would not have thought, that in the great heats of this ſummer the burning mirror of the royal palace would have produced greater effects than at any other time? but it was quite contrary, and certainly that could not have been gueſſed by any ſyſtem. M. *Homberg* ſaw, that the rays of the ſun, when reunited by the mirror, had hardly any ſtrength, while the direct rays alone inflamed the air. The reaſon which he imagines for ſuch a ſurpriſing *phœnomenon* is, that great heat raiſes from the earth an infinite number of ſulphurous exhalations, and that theſe matters, through that homogeneity which they have, according to M. *Homberg*'s ſyſtem, with that of light, hinder, ſtop, and in ſome ſort abſorb the rays, whether it be that they abſolutely intercept a part of them, and hinder them from falling on the mirror, or that they have the ſame effect on them, which a ſcabbard has on a ſword, and thereby take from them that extreme ſubtilty, which is neceſſary to cut hard bodies readily. This conjecture is confirmed by an experiment made by M. *Homberg*. He placed between the mirror and the *focus* a chaffindiſh full of live coals, ſo that thoſe rays, which reached the *focus*, paſſed thro' the vapours of theſe coals, and he ſaw that the mirror was conſiderably weakened by it. This is a repreſentation of what happens in great heats, or rather, the very thing in miniature. Alſo M. *Homberg* has always obſerved, even in moderate and common heats, that when the ſun has appeared many days together, the effect of the mirror is not ſo great, as when the ſun firſt appears immediately after a great rain. The reaſon is, that this rain hath precipitated the ſulphurous matters, and cleanſed the air. That trembling of the light, which has always been obſerved with large teleſcopes, and which in very great gnomons makes the end of the ſhadow uncertain, is very naturally explained, by the ſyſtem of M. *Homberg*, and is a new proof of it.

On this reflection may be made, that the burning mirror, which is a new furnace for chymiſts, infinitely ſuperior to the old common furnaces, hath this inconvenience, that it cannot be but ſeldom made uſe of, at leaſt with full force. It muſt be in the ſummer, between the hours of 9 and 3, the ſun muſt ſhine, no clouds muſt paſs over it during the whole time of the operations, the days muſt be of a moderate heat, and muſt not have been preceded by many dry days. There are ſome years, in which it would be difficult to find a week of ſuch days.

XII. *Of the yellow amber; tranſlated by Mr.* Chambers.

The marquis *de Bonac*, the king's envoy extraordinary at the court of *Sweden*, having ſeen ſome foſſil amber in the lands of M. *Grata*, poſtmaſter-general of *Pruſſia*, near the city of *Dantzic*, which appeared of the ſame nature, as that found on the ſea-coaſt, began to conſider that mixed more narrowly than before, and to doubt whether it were really form'd of the froth of the ſea, as is commonly ſuppoſed; and the cardinal primate of *Poland*, who was with him, and had the like curioſity, moved him to aſk the opinion of the academy hereupon. M. *de Bonac* wrote accordingly to *Paris*, and the academy was induced hereby to collect all they could learn on this head, the reſult whereof was remitted to the marquis in the following memoir.

As not only the fineſt but the greateſt quantity of amber comes from the two *Pruſſias*, the academy is leſs acquainted therewith, than thoſe perſons may be, who are pleaſed to conſult it; yet we ſhall ſpeak what we can learn of it from our own knowledge, and add ſome reflections thereon, without enquiring what other authors have wrote of it, as taking it for granted thoſe authors are known, and that it is not a collection that is rerequired of us. Meſſ. *Caſſini* and *Maraldi*, travelling in 1700 into the the ſouthern provinces of *France*, on account of continuing the meridian of *France*, found ſeveral mines of jet, and one of a kind of amber, in a mountain of *Languedoc Bugarache*, which is upwards of 27600 fathoms from the ſea, and divided by other high mountains therefrom. Some take jet, as well as yellow amber, for a ſpecies of *ſuccinum*; and 'tis certain the inhabitants of *Bugarache* make uſe of their amber to burn in their lamps. 'Tis much like a reſin, and inferior by many degrees to the *Pruſſian* amber in hardneſs; near the mines of *Bugarache*, there are ſprings of ſalt-water, which form a little river.

In the hiſtory of the academy for the year 1700, 'tis mentioned, that there is amber found in the clefts of the moſt barren and naked rocks of *Provence*, which is further confirmed in the hiſtory for 1703 *; and we are aſſured, by the moſt creditable relations, that there is amber likewiſe found in *Sicily* along the coaſts of *Agrigentum*, *Catanea*, and *Leocata*, as alſo in the iſland of *Corſica*, and even about *Bologna* in *Italy*, and in *Umbria*, far diſtant from the ſea; add to this, that M. *de Bonac* himſelf relates, that he had ſeen amber dug out of the grounds of M. *Grata*, which were divided from the ſea by large woods and great mountains; all which ſeems to evince, that this matter is a natural production of the earth, nor muſt it be omitted, that we find little animals incloſed in amber, and that theſe animals are always of the terreſtrial kind, as flies, piſmires, *&c.*

However, for the greater ſecurity, it might be proper to examine, whether the terreſtrial ambers have all the characters and perfections of the amber found on the ſea ſhores; for 'tis no way impoſſible, but that the ſea may finiſh the preparation of this matter, and contribute by its ſalt to give it the laſt degree of coction.

Suppoſing, however, that amber were always produced by the earth, at leaſt its origin, or firſt formation, it remains to be enquired, whether it be of the vegetable or mineral nature.

We have never heard of any trees in *Pruſſia* that diſtil amber, or any thing like it in form of a reſin; and yet it would be more natural to ſuppoſe, that the ants and flies ſometimes ſeen in it, and which plainly confeſs it to have once been liquid, had been incloſed in a reſin oozing from ſome tree, than in a mineral formed in the ground. To get over this difficulty, we muſt ſuppoſe that the amber had trickled from ſome rock, or at leaſt that ſuch pieces, wherein theſe animalcules are found, had remained ſometime fluid on the ſurface of the earth.

---

* See pag. 7 of this volume.

But whether we ſuppoſe amber to be vegetable or mineral, no body pretends to have ever ſeen it liquid, or barely ſoft; and yet it muſt have been both, and even expoſed to ſight at the time when the animals were buried in it.

The analyſis of this ſubſtance made by the chymiſts of the academy, does not finally determine which claſs it belongs to. They have always found a very ſmall quantity of aqueous liquor in it, which had the ſmell of rubbed amber, a great quantity of volatile acid ſalt, and ſtore of oil, part of it whitiſh like water, part browniſh, and part very black, according to the degrees of fire that had been given it in the diſtillation. The *caput mortuum* left behind was light, ſpongeous, black, and gloſſy; and, being calcined by a naked fire, went moſtly off in ſmoak, and yielded no fixed ſalt.

The only difference between the analyſes of different ambers conſiſts in this, that the moſt tranſparent and whiteſt yield more oil and volatile ſalt, and leſs *caput mortuum*, than the darker kind; which laſt, however, notwithſtanding their ſtore of *caput mortuum*, afforded no fixed ſalt.

The oil of amber has the ſmell of a bituminous oil, which ſhould indicate amber to be a bitumen; but there are ſome reſins, whoſe diſtilled oil has the ſame ſmell; and there are others, as benjoin, which yield a volatile acid ſalt.

But we know of none, which at the ſame time yields both a volatile acid ſalt, and an oil with a bituminous ſmell; whence the academy is moſt inclined to take amber for a bitumen, and conſequently for a mineral.

'Tis eaſy to ſee how many things are ſtill wanting to enable the academy to form a more preciſe deciſion upon what relates to amber: it ſhould be known,——1ſt, Whether in the neighbourhood of the places where the amber is dug, there be not ſome ſaline or vitriolic water. 2dly, Whether it be commonly incloſed or mixed with any earth, or other particular matter. 3dly, Whether there be any marks whereby to diſtinguiſh the places, where the amber is found. 4thly, Whether foſſil amber differs in any thing from that found about the ſea-ſhore. 5thly, Whether any white, as well as yellow amber, be found on the ground, and whether it be not the air or the ſun's heat, that converts the yellow into white. 6thly, Whether in thoſe places where yellow is dug, there be not likewiſe black amber found. 7thly, Whether it be true, which *Hartman* in his hiſtory of the *Pruſſian* amber, and *Bartholin* on the *Daniſh*, relate, that it is found under a kind of foliated earth, not unlike the barks of trees, and that it is accompanied with a ſort of foſſil wood, wherein, however, we neither perceive pith, fibres, knots, nor buttons.

XIII. *Of M.* Deliſle's *map of the* Roman *empire.*

A great part of the ſtates which at preſent compoſe the known world, were formed on the ruins of the *Roman* empire, diſmembered and torn to pieces by *Barbarians*. As it is from thence that our modern hiſtories take their original, and as it is they, which moſt concern us, M. *Deliſle* hath prepared a map, which muſt be a great help to the underſtanding them well. It contains not only the *Roman* empire, but alſo all the barbarous countries which ſurrounded it, a little time before the people of thoſe countries had made any breach therein by their invaſions. His epocha is the year 400 of the chriſtian æra. M. *Sanſon*, a celebrated geographer, had already made a map of the *Roman* empire, which was very much eſteemed in his time; but he did not include therein the barbarous countries, the poſition and determination of which muſt be as laborious as it is inſtructive. M. *Deliſle* hath named his map *theatrum hiſtoricum*, becauſe of the great extent it takes beyond the *Roman* empire, and the uſe it is of to our hiſtories.

Beſides the earth has been much altered ſince M. *Sanſon*'s time, that is, the aſtronomical obſervations both more exact and in a greater number have produced great reformations in geography. They were much miſtaken about the longitudes, which are naturally more difficult to determine than the latitudes, and were often miſtaken about the latitudes themſelves; and M. *Deliſle* was obliged to differ always from M. *Sanſon* on the firſt of theſe meaſures, and often on the ſecond, which entirely changes the figure of the countries, ſeas, *&c.*

It is an obſervation, which is not altogether new, that the errors in geographical meaſures have always hitherto conſiſted in the exceſs.

From the time of the *Greeks* to us, the earth hath always diminiſhed each time they have undertaken to diſcover the bigneſs of it.

From thence it proceeds, that though the ſame *Roman* empire, or the ſame countries, are by M. *Sanſon* in a larger map than that of M. *Deliſle*, and that conſequently the ſcale of M. *Deliſle*'s map ought to be the leaſt, yet it is more than a fifth part larger. The reaſon for which is, that in

M. *Sanſon's* map, that the *Roman* empire is much too large, when compared with the reſt of the ſurface of the earth. The perfection of maps depends on the exact proportion of the parts of this ſurface among themſelves, and we cannot hope to know it but by aſtronomy, which daily diffuſes a greater light on geography.

AN ABRIDGMENT OF THE PHILOSOPHICAL MEMOIRS of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1705.

I. *Obſervations on the quantity of rain-water, which fell at the royal obſervatory during the year* 1704, *with the heights of the barometer and thermometer, and obſervations on the winds that reigned, by M.* de la Hire *.

DURING the year 1704, the water which fell was pretty equally diſtributed through all the months, except thoſe two of *July* and of *October*, in which it rained very little; the dry weather of this laſt month is very uſeful for the better ſowing of ſeeds.

This is the quantity of water during each month.

	Lines.		*Lines*
Jan.	15	July	9 ¼
Feb.	15 ½	Aug.	27
Mar.	19 ¼	Sept.	34
Apr.	16	Octob.	8 ¼
May	27 ¼	Nov.	19 ¾
June	24 ¼	Dec.	23

The ſum of water in the whole year 238 ½ lines, or 19 inches 10 lines, which is very near the 19 inches, which we have determined for the mean quantity of water which falls each year.

Jan. 10, 1705.

On

On the winds.

Throughout the whole month of *January* the wind reigned towards the N. inclining at the beginning towards the E. and at the end towards the W. there was no rain from the 10th to the 24th.

In *February* the wind was almoſt always in the W. and ſometimes in the S.

In *March* the wind was almoſt always in the S. in the beginning it inclined to the W. and at the end towards the E. there was no rain from the 15th to the 3d of the month following.

In *April* the wind was the ſame, except in the laſt days, when it turned towards the N.

In *May* the wind was very variable.

In *June*, the wind was in the beginning between the N. and E. and at the end towards the W.

In *July* the W. wind governed, and it rained no more than 4 lines from the 27th of *June* to the 28th of this month.

In *Auguſt* the wind parted from the E to N. and afterwards to the W.

In *September* the wind was almoſt always S. W.

In *October* the principal wind was the N. inclining towards the E. and ſometimes to the W. From the 4th of this month to the 27th, it rained but one line.

In *November*, the wind was in the beginning towards the N. and in the middle and to the end towards the S. W.

In *December* the principal and governing wind was the S. W. It is ſeen by all theſe obſervations, that the wind which reigned moſt was the W. as it almoſt always happens in theſe countries; and it is alſo from theſe ſorts of winds that it moſt commonly rains; but the greateſt rains, which did not exceed the height of an inch, came with a northerly wind. There has been no conſiderable ſtorms during this year.

On the barometer.

What is moſt conſiderable in the barometer, and which ſhews us the weight of the air, are the changes which happen to it in 2 or 3 days, in which time we ſee it often riſe and fall more than an inch; which makes us acquainted with the great variations which happen in a little time as to the height of the atmoſphere. For to give a reaſon for theſe different weights of the air, it does not ſeem to me very likely to ſuppoſe, as do ſome philoſophers, that there are different liquids and a different weight on the ſurface of the earth, which are ſometimes carried on the one ſide, and ſometimes on the other; for they muſt be commonly more light when the air is moſt loaded with vapours, as is known by obſervations.

Methinks all that is obſerved in the weights of the air, or of the atmoſphere in all its circumſtances, may be very well explained in the manner following. We know by very exact obſervations, that the barometer generally riſes to a leſs height between the tropics, than in the northern countries; whence it may be conjectured that the figure of the atmoſphere is a long ſpheroid, whoſe axis is joined to that of the earth, which may eaſily be explained in the ſyſtem of *Copernicus*. But as wherever there is air there may be winds, if the ſame winds reign through all the maſs of air, and if it comes from the S. it will lower the height of the atmoſphere in theſe countries; and on the contrary, if it comes from the N. it will raiſe it. But alſo as the S. winds bring us rain, it will follow, that it ought to rain when the air appears light, and the quite contrary will happen when it is on the other ſide.

This is in general what ought to follow this ſuppoſition; but if the S. wind reigns only on the ſurface of the earth, and that there is a N. wind in the ſuperior part, it may rain altho' the air appears very heavy, and by a contrary reaſon the weather may be very ſerene with a N. wind, and the barometer very low; for we can only obſerve thoſe winds which are near the earth.

During this year the barometer roſe pretty often beyond 28 inches; but on the 25th of *December* in the morning it roſe the higheſt to 28 inches 3 lines $\frac{1}{6}$; and the loweſt was *Nov.* 25th, to 26 inches 11 lines, by the height of the great hall of the obſervatory where my barometer is placed. The whole difference of height between the higheſt and loweſt has therefore been 1 inch 4 lines $\frac{1}{6}$. No concluſion can be made from the winds which have reigned in the greater or leſſer heights of the barometer, for the reaſons which I have above related, ſince we can only obſerve thoſe winds which are on the ſurface of the earth. I have only taken notice that it did not rain when the barometer was at the higheſt, and that there was much rain when it was at the loweſt, ſometimes with a N. and ſometimes with a S. W. wind.

On the thermometer.

My thermometer fell loweſt on the 23d of *January* to 14 degrees $\frac{4}{5}$. Its mean ſtate, ſuch as it is, in the bottom of the caves of the obſervatory, is 14 toiſes below the banks level, being at 48 degrees, and the froſt beginning when it is at 32 degrees; but it immediately roſe again near the 30 degrees. The heat was greateſt the 28th of *July*, the thermometer riſing to 66 degrees ¼. Theſe obſervations on the thermometer are always made near the ſun's riſing, which is the time of day wherein the air is moſt cold.

It is ſeen thereby that the cold has been very near in the ſame degree as the heat, compared with a mean ſtate, if the 23d of *January* is excepted. Alſo in the day-time, and about two in the afternoon the heat is much greater than in the morning, and I have found the thermometer at 75 degrees ſhaded from the ſun ; and conſequently there has been more heat than cold this year in theſe countries.

On the declination of the needle.

I have obſerved the declination of the needle the 30th of *October*, 9 degrees 20 minutes towards the weſt, with the ſame needle of 8 inches length, and in the ſame place where I uſed to obſerve it.

II. *A compariſon of the obſervations on the rain and winds made by M.* de Pont-briant *at the caſtle of* Pont-briant, *two leagues from* S. Malo, *and towards the ſea-coaſt, during the year* 1704; *with thoſe which were made at the obſervatory at the ſame time, by* M. de la Hire *.

Theſe obſervations which have been made in *Bretagne* with great exactneſs, having been communicated to the academy by M. *du Torar*, to whom M. *de Pont-briant* had ſent them ; it has been thought proper to compare them with thoſe which were made at *Paris* at the ſame time, the journal of which I have already given. Here is only given the quantity of rain which fell during each month; but it muſt be obſerved, that it rained very often at the ſame time in theſe two places, which are diſtant about 80 leagues, one of which is weſtward from the other, and almoſt in the ſame parallel; but ſtorms are much more frequent at *S. Malo* than at *Paris*.

---

* Feb. 25, 1705.

At Paris.	Lines.	*At Pont-briant* Lines.
January	15	11 ¾
February	15 ½	22 ½
March	19 ¼	25 ¾
April	16	21 ¼
May	27 ¼	17
June	24 ¼	2
July	9 ¼	13 ¼
Auguſt	27	27 ½
September	34	51
October	8 ¼	18 ¼
November	19 ¾	57 ¾
December	23	25 ¼

Sum of the water at *Paris* 238 lines ½,
at *Pont-briant* 284 lines, or
19 inches 10 lines ½.
23 —— 8 —— ¼.

It is hereby ſeen that the quantity of rain in each month, has not much differed, except in *September* and *November*, wherein it rained much more at *Pont-briant* than at *Paris*. And in the month of *June* it rained much leſs at *Pont-briant* than at *Paris*; but the one does not make amends for the other, for there fell near 4 inches more water at *Pont-briant* than at *Paris*, altho' in *Paris* the quantity was very near the ſame as in the mean years.

It was ſome years ago, that the marſhal *de Vauban*, who is now preſident of the academy, cauſed the ſame obſervations to be made in the citadel of *L'Iſle* in *Flanders*. I then compared them with thoſe of *Paris*, and I found that it commonly rained a little more in *Flanders* than at *Paris*.

By the obſervations of the winds made at *Paris* and at *Pont-briant*, it is remarked, that the wind is not commonly the ſame in both places, and that it always inclines more to the S. at *Paris* than at that place. As to the rains which accompany the winds, there is much variation in the times and years. Not but that in general it is found by this year's obſervations, that at *Pont-briant* great rains, with ſtorms, have been always accompanied with a N. W. wind, and ſometimes a N. but ſeldom a N. E. At *Paris* they come almoſt always from the S. W. The neighbourhood of the ſea at *St. Malo*, and the diſpoſition of the *Engliſh* channel, in regard to the coaſt of *Bretagne*, may cauſe this difference, both for the direction of the wind and of the rain.

It is no wonder that the winds differ in places very little diſtant compared with the whole ſurface of the earth, ſince we ſee frequently, that in the ſame place there are different winds which reign in the air, and ſometimes are quite oppoſite, which may be obſerved by the motion of the clouds. One of the winds may begin in one place, and the other in another, either more or leſs diſtant from the ſame place. Theſe winds at length mixed make but one mean, where one gains the aſcendant prevails over the other; and it may happen, that this ſtruggle between theſe contrary winds, when they are very violent, may cauſe ſtorms and hurricanes.

M. *de Pont-briant* obſerves in the letter which he wrote to M. *du Torar*, that it freezes much leſs at *St. Malo* than at *Rennes*, but the cauſe of that ought to be attributed only to the proximity of the ſea: for the great quantity of vapours, which ariſe from the ſea-water, and may retain ſome marine ſalts, may prevent the freezing, ſince it is known by experience, that ſea-water does not freeze ſo eaſily as freſh water, and that water, wherein a little marine ſalt has been diſſolved, will not eaſily freeze. I have alſo formerly obſerved at *Breſt*, that they preſerved ſome *ananas's*, or pine apples, in the open ground during the whole winter, though they were expoſed to the air.

III. *Reflections on the obſervations of the variation of the needle, made in the pope's legate's voyage to* China, *in the year* 1703, *by M.* Caſſini *the ſon* *; *tranſlated by Mr.* Chambers.

A mercator's chart was lately ſent us from *Pondichery*, by M. *de May*, a miſſionary, who went with M. *de Tournon*, the pope's legate to *China*; in which chart is expreſſed by dotted lines the courſe which the ſhip *Maurepas* made each day from the *Canaries*, which they left on the firſt of *May*, 1703, to *Pondechery*, where they arrived on the 6th of *November*, after a voyage of upwards of 6 months.

In this voyage, they obſerved the variation of the needle in ſeveral places by the riſing and ſetting of the ſun, and took care to expreſs the ſame on the chart, in the proper place of their rout for the day of obſervation.

---

* Jan, 10, 1705.

As the new chart of the variation, drawn by Dr. *Halley* for the year 1700, comprehends all the places expreſſed in this rout, we took occaſion hence to examine, whether it would agree with theſe new obſervations; and in order hereto laid down all the places, where M. *de May* made his obſervations, and Dr. *Halley*'s chart, allowing for the different longitudes on which the two charts are conſtructed. ——— We proceed now to make a compariſon.

On the 18th of *May*, 1703, at 358° ——— of longitude, and 5°———40′ northern latitude, the variation was found by the ſun-ſetting to be 1° ½ from the north towards the weſt.——— Now the place where this obſervation was made, being laid down on Dr. *Halley*'s chart, is found a little to the weſtward of the line of no variation, and on that ſide where the variation begins to be eaſtwards; ſo that by this obſervation it appears, that the line of no variation ought in ſtrictneſs to be a little to the weſt of the place, wherein it appears in Dr. *Halley*'s chart, which agrees likewiſe with what had been before obſerved in a memoir for the year 1704.

On the 6th of *June* at 356°———longitude, and 5°———20′——— ſouthern latitude, the variation was found by the ſun's riſing to be 1°——— eaſtward of the north, which agrees near enough with Dr. *Halley*'s chart, wherein this place is repreſented between 1 and 2° ——— of variation eaſtward.

On the 11th of *June* at 352°—40′ longitude, and 11°—15′ ſouthern latitude, the variation was found 1°——½ eaſtward of the north, which in Dr. *Halley*'s chart is repreſented as a little more than 3°——

On the 19th of *June* at 1°—— to the ſouth of the moſt ſouthern part of the iſland of *Aſcenſion* being 350°—— of longitude, and 21°—0′—of ſouthern latitude, the variation was found to be 6°——½ eaſtward of the north, where in Dr. *Halley*'s chart it is repreſented as 7°—½.

On the 3d of *July* at 7°—45′—longitude, and 34°—40′—ſouthern latitude, the variation was found 3°—¼ eaſtward of the north, being much the ſame as in Dr. *Halley*'s chart.

On the 8th of *July* at 24°—10′— longitude, and 36°— ſouthern latitude, the variation was found 3°—weſtward of the north, the ſame is repreſented in this place by Dr. *Halley* between 3 and 4°—

According to theſe two laſt obſervations, in one whereof the variation is found eaſtward, and in the other weſtward, both of them agreeable to what are repreſented in Dr. *Halley*'s chart, the line of no variation croſſes M. *de May*'s courſe, much in the ſame place where Dr. *Halley* makes it croſs.

On the 13th of *July* a degree to the ſouth of the *Cape of Good Hope*, at 41°— longitude, and 36°—20′—— ſouthern latitude, the variation was found 13°— weſtward of the north, which in Dr. *Halley*'s chart is noted 11°——

On the 19th of *July* at 53°——20′ longitude, and 35°——35′—— ſouthern latitude, the variation was found 19°— weſtward of the north, exactly the ſame as in Dr. *Halley*'s chart.

On the 25th of *July* at 69°— longitude, and 32°—50′ ſouthern latitude, the variation was found

25°—½ weſtward of the north; in Dr. *Halley*'s chart it is between 24 and 25.

On the 12th of *September* at 98°—30′ longitude, and 28°— ſouthern latitude, the variation was found 19°— weſtward of the north exactly, as repreſented by Dr. *Halley*.

On the 17th of *September* at 96°———35′ longitude, and 22°—40′ ſouthern latitude, the variation was found 15°— weſtward of the north, which in Dr. *Halley*'s map is repreſented between 15°— and 16.

On the 2d of *October* at 106°—40′ longitude, and 1°—20′ ſouthern latitude, the variation was found 4°— weſtward of the north, which in Dr. *Halley*'s chart is found between the 5th and 6th degree.

Laſtly, on the 2d of *November* at 105°—20′ longitude, and 14°—40′ ſouthern latitude, the variation was found 4°———45′, preciſely the ſame as in Dr. *Halley*'s chart.

Upon the whole, it appears, that ſome of theſe obſervations fix the variation exactly the ſame, as is done by Dr. *Halley*, that the greateſt part of them do not deviate from it a degree, and that none of them vary 2°: ſo near a conformity muſt be allowed very conſiderable, conſidering the difficulty of obſerving the variation with exactneſs at ſea, and the changes which may have happened within the three years elapſed between the drawing of Dr. *Halley*'s chart, and M. *de May*'s voyage.

I know not whether Dr. *Halley* had any further view in his chart, beſides determining the variation of the load-ſtone for the conveniency of ſailors; but if in other courſes and voyages, the agreement ſhould be found as exact as in the preſent one, it might likewiſe be of uſe for determining the longitude at ſea, eſpecially on the other ſide the equator; for the lines which ſhew the variations from degree to degree, cut the parallels in thoſe parts pretty directly, and are likewiſe very near to each other, as appears in the preſent courſe from the line of no variation to that where the variation is 25°— which here correſpond to 34°—— difference of longitude.

In effect, almoſt all the places where M. *de May* obſerved the variation may be laid down in Dr. *Halley*'s chart, by the interſections of parallels with the lines which denote the variation obſerved; yet without finding any other differences than what may well be attributed to the difficulty of determining the longitude of the place where the obſervation is made.

The variation being once well eſtabliſhed, it were to be wiſhed, that a rule could be found for the changes or variations which befal the ſame in a compaſs of time; but to arrive hereat, a great number of obſervations will be required, made with the utmoſt care, and at conſiderable diſtances of time, which is an advantage we are hitherto deſtitute of: for tho' father *Riccioli* made a large collection of ſuch obſervations, yet having omitted the names of moſt of the obſervers, as well as the times when they were made, we cannot make much of them.

The chief aſſiſtance we have in this caſe, is from ſome obſervations made by the jeſuits in their voyage to the *Eaſt-Indies*, related by father *Gouye*, in the phyſical obſervations for the year 1692, which will inform us of ſome changes, that the declination has undergone.

Father *Noel*, in his way to *China* in 1684, obſerved, that at 215 leagues weſtward of the *Cape of Good Hope*, the needle had no declination.

tion.—— Agreeably to which obſervation, the line of no variation muſt have then been conſiderably to the eaſt of the place, wherein it is repreſented by Dr. *Halley*, and where it ought to be now placed, according to the obſervations of M. *de May*, who in 1703 found the variation in the ſame place 3°——— weſtward of the north.

In 1684, the ſame father found the variation at the *Cape des Eguilles* 10°——— weſtward of the north, which in M. *de May*'s chart is repreſented as 13°——— From both which obſervations we have an augmentation of 3°——— in 19 years, which is at the rate of 10′——— *per* year.

In *Ricciolus*'s collection, we have no obſervation of the declination at this *Cape*; which, 'tis probable had none when it was firſt named *Cape des Eguilles*, that is, of the needles; but in the 8th book of his geography, he gives ſeveral obſervations made in the neighbourhood of that *Cape*; and among others, one of *Gerard de Dieppe*, who, in 1639, found the variation 14 leagues beyond the *Cape of Good Hope*, which muſt be nearer the *Cape des Eguilles* 1°———½ weſtward. —By a compariſon of which, with an obſervation given by Dr. *Halley*, we have an alteration of 11°———½ from the north to the weſtward, in the ſpace of 64 years, which amounts to ſomewhat leſs than 11′——— *per* year.

Father *Noel* further obſerves, that the *Portugueſe* pilots affirm, that from the *Cape des Eguilles* to *Madagaſcar*, the declination increaſes 13°— weſtward, and conſequently if it be 2 —— at the *Cape*, it will be 15 at *Madagaſcar*, which agrees likewiſe with the declination expreſſed in the new chart, which is 13°——— at the *Cape des Eguilles*, and 25—½ under the meridian of *Madagaſcar*.

From *Madagaſcar* to *Pondichery* the declination diminiſhes; ſo that in M. *de May*'s map, a little to the eaſt of *Pondichery*, it is expreſſed as 4°——45″ weſtwards of the north. Now the declination at *Pondichery* was found by father *Richaud* in 1689, to be 7°—0″— From whence it appears, that the declination had diminiſhed 2°—¼ in 14 years, which is at the rate of 10′—*per* year; whereas at the *Cape des Eguilles*, we had before found an increaſe of the like quantity; and it may be added, that father *Richaud* found at *Louvo*, in the ſpace of two years, a diminution, like that here noted at *Pondichery*.—From the whole therefore it may be conjectured, that in the *Eaſt-Indies*, from the meridian of *Madagaſcar* eaſtward, the weſtern declination diminiſhes every year in the ſame proportion, as it increaſes from the ſame iſland eaſtward.

IV. *Of barometers without quickſilver for the uſe of the ſea, by M.* Amontons *.

If air is incloſed in a glaſs ball D †, joined to a glaſs tube E, C, B, A, bent at C, open at A, and containing a liquor from the entrance E of the ball, to ſome place of its part A B; it has been long ſince known that this air incloſed in D, will augment or diminiſh its bulk, not only in proportion as the heat of the outward air changes, but alſo in proportion as the weight of it changes; mean while I know not whether any one has yet diſtinguiſhed and determined the quantity of theſe two effects, I mean, how much the heat and the weight of the outward air, acting in conjunction on that which is contained in D, will, each in their particular, make this bulk of incloſed air diminiſh or increaſe; in a word, what would be the

---

* Feb. 11. 1705. † Plate V. Fig. 1.

the motion of the liquor in the tube A B. Theſe two effects have always appeared difficult to be ſeparated from one another, becauſe of the combination of many circumſtances, which make them vary almoſt an infinite number of ways.

As to what concerns the effect of the weight of the atmoſphere on this very air; indeed M. *Mariotte* has already given us ſome experiments and rules thereon; but it no where appears that his deſign was by this means to meaſure the viciſſitudes of the weight of the atmoſphere, by preventing us from attributing the effect of the weight to that of heat, and reciprocally that of heat to the effect of the gravity. As that may nevertheleſs have its uſe, I am endeavouring to do it in the beſt manner I can poſſibly, continuing to make uſe of what M. *Mariotte* has ſettled thereon. Now by the ſame experiments it is clear, that the more conſiderable the bulks of air in D are, the more will the liquor in the tube A B ſink or riſe, by one and the ſame over-charge, or by one and the ſame diminution of the weight of the atmoſphere; and that if this liquor in A B, had no weight, the bulks of incloſed air would in their changes follow the proportions of the weights which they are loaded with, ſo that theſe bulks would be in an inverted ratio of theſe weights.

* Thus then ſuppoſing the ball D lengthened out into a long very thin eylinder of the ſame ſize with the tube A C, and the whole in a horizontal ſituation to avoid the weight of the liquor; if this ball thus lengthened was, for example, 14 feet long, and the liquor in E was at the beginning of theſe 14 feet, when the weight of the atmoſphere equals 26 inches of quickſilver; this liquor would advance a foot, when the weight of the atmo-

* Fig. 2.

ſphere is at 28 inches, theſe bulks 14 and XIII. being by the inverſed ratio of the weights 26 and XXVIII; and the ſame change of the weight of the atmoſphere would have made the liquor advance differently according as the lengthened ball had more or leſs capacity or length. Thus it would have advanced two feet, if the length of the lengthened ball had been 28 feet, inſtead of 14; and 4 feet if this lengthening had been 56 feet; and ſo of the reſt. Whence it appears that the effect of the weight of the atmoſphere on the air in the ball D, becomes always greater and greater, according as the greatneſs of this ball increaſes; which cannot be ſaid of the effect of heat, which, as I have already ſhewn elſewhere, would be always equal, notwithſtanding the augmentation of theſe bulks. The ball D may then be ſuppoſed ſo prodigiouſly great, that the effect of the changes of the heat of the atmoſphere, ſhould not be diſcerned in compariſon of the effect of the changes of its weight; which ſtill ſuppoſes the tube A B to be in a horizontal ſituation. But as ſuch a ſituation is inconvenient for uſe, and that it would be better to have it vertical, in this ſituation the liquor cannot paſs from the tube A C, into C D, or out of that into the other, without leſſening the impreſſion of the weight of the atmoſphere, againſt the air incloſed in D, or of this againſt the atmoſphere; and that ſo much the more according to the greater weight of the liquor which ſhall be made uſe of. Thus, for example, if the weight of this liquor is to that of the quickſilver as 1 to 14, and if a quantity of this liquor contained in A B, in the compaſs of 28 inches, paſſes into the ball D toward E, it is plain that this ſinking of 28 inches of liquor will equal the effect of the atmoſphere, whoſe weight will be encreaſed to a quantity equal to two inches height of quickſilver; and as we know experimentally, that the motion of the ſimple barometer, which is cauſed by the more or leſs weight of the atmoſphere, does not here exceed the extent of two inches; it is alſo evident, that the range of the liquor in the tube A B, ſituated vertically, cannot by the change of the weight of the atmoſphere, exceed with a like liquor 28 inches, let the ball D be never ſo large: it cannot even, taking it rightly, go ſo far, becauſe the ſpring of the air in D will always make ſome reſiſtance to the diminution of its bulk, let this diminution be never ſo little; and let the tube A B be never ſo ſmall, and conſequently how little ſoever the quantity of liquor is, which is contained in the compaſs of 28 inches of this tube, it is impoſſible that this quantity of liquor paſſing from A B into E, ſhould not ſomething diminiſh the bulk of air in D. The compaſs of this range of the liquor in the tube A B, will be even conſiderably leſs than 28 inches, tho' the ball D ſhould be only of a moderate ſize; and experience has ſhewn me, that with a liquor, whoſe weight is to that of quickſilver, near as 1 to 14, the extent of this range can be but 20 inches, with balls of two inches diameter; and only 16 inches, with balls of 1 inch $\frac{3}{4}$; which would ſtill be leſs if the liquor was more heavy. But as on the contrary, thoſe which are more light, may very well be employed in it; and as already this range of 20 inches is at leaſt as conſiderable as that of the double barometer of M. *Huygens*; nothing hinders the tubes A C D from being ſerviceably made uſe of, in which there muſt be liquor from the middle of the part A B to E, to know by the motion of the liquor in A B, the changes of the atmoſphere, in the ſame manner as is done with common barometers; and ſo much the better, as being more portable, and not ſo ſuſceptible of motion, they may very well be made uſe of at ſea, where the ſhaking of the veſſel will not hinder an exact obſervation of the diverſe changes; which cannot be done with the common ones. Having found that the extent of the range of the liquor in theſe tubes by the changes alone of the weight of the atmoſphere, were conſiderable enough to make uſe of it as a barometer; and after having divided this extent into 24 equal parts, to make thereof a graduation, which may deſcribe the quantities of quickſilver, which equal the weight of the atmoſphere in all its changes; it now remained for me to apply this graduation to theſe new barometers. This did not at firſt appear to me very eaſy, becauſe of the action of heat, which continually changing, did not permit me to be able to aſſign on theſe tubes any fixed place for this graduation. But having conſidered that even this which ſeemed an obſtacle to me, might ſerve me for a direction, this graduation being always to follow the motion which heat ſhould cauſe in the liquor, and that when heat cauſed no motion in it, this graduation ought alſo to remain in the ſame place; I made it moveable in the following manner.

I put one of theſe tubes for a conſiderable time near one of my thermometers, and I obſerved the range of both, at times when I was aſſured by the obſervation of the barometer, that the weight of the atmoſphere had not changed; which gave me the means of making on the ſide of this tube a graduation, like thoſe of my thermometers, though greater. This graduation deſcribed the changes which heat cauſed in the height of the liquor of this tube. After this I put on the ſide of this graduation of the effect of the heat, the graduation which I had firſt made of the effect of the weight of the atmoſphere; ſo that I could raiſe it or lower it as I thought fit, and bring the middle of it to ſuch a degree with that of the heat as I pleaſed; and when I would know the weight of the atmoſphere, I looked firſt to ſee what degree my thermometer was at, and afterwards I carried the middle of the graduation of the barometer to the ſame degree of that which I had made on the ſide of the tube, to deſcribe the changes cauſed by heat on the liquor of the tube, which then ſhewed me by the moveable graduation, the weight of the atmoſphere which I ſought after.

Having afterwards proved theſe obſervations for a conſiderable time on my rectified barometer, I can affirm that I have always found both of them exactly the ſame. This will more eaſily be believed, if it is conſidered that no quickſilver enters into the conſtruction of theſe new barometers, and that the heat acts very weakly on the liquor which they contain, which is alſo but in a very little quantity; which is the reaſon why theſe barometers muſt be exempt from thoſe defects which I have obſerved in the common ones where quickſilver is employed. It is true that the graduation of theſe new barometers, which are to include the effect both of heat and of the weight of the atmoſphere, obliges them to be made of more than ordinary height; but this cannot go ſo far as to make them uſeleſs; thoſe whoſe balls have two inches diameter, being not requiſite to be more than 5 feet long, and the others only 4 feet, which is but about 10 inches more than the common ones when they are mounted; and this ought not to hinder the endeavours of making them ſome way uſeful to navigation by the obſervations which may be made by them at ſea.

V. *Reflections on the rules for the condenſation of the air, by M.* Caſſini *the ſon* *; *tranſlated by Mr.* Chambers.

In our travels for continuing the meridian of *Paris*, we obſerved the height of ſeveral mountains by the edge of the ſea, and among others that of *Puy de Dome*, where M. *Perier* made obſervations of the height of the mercury, mentioned in M. *Paſchal*'s treatiſe of the *equilibrium* of fluids, which obſervations having ſerved M. *Mariotte* for confirming his rules of the condenſation of air, I took occaſion to compare his rules with my own obſervations.

M. *Mariotte*, in his ſecond eſſay of the nature of air, gives us ſome experiments, which he made to determine the condenſation of air; from whence he infers, that it may be taken for a certain rule or law of nature, *that the denſity of air is in proportion to the weight wherewith it is preſſed.*

On this principle he afterwards determines in a very ingenious manner, the height of the atmoſphere to be about 15 leagues of 2000 fathoms each.

He ſuppoſes that mercury in its natural ſtate, on a level with the ſea, is ſuſtained in a barometer at the height of 28 inches, which make a balance to the whole column of the atmoſphere, and that in this caſe a line of mercury ſuſtains 60 feet of air, and the 12th part of a line 5 feet.

---

* March 24, 1705.

If we ſuppoſe the mercury removed into a higher place, where it is only ſuſpended to the height of 14 inches, it will not ſuſtain above $\frac{1}{2}$ the weight of the atmoſphere; and conſequently the air, which, according to M. *Mariotte*'s rule, condenſes in proportion to the weight wherewith it is preſſed, ſhould here be rarer by $\frac{1}{2}$, than in the former caſe; and a line of mercury, which on a level with the ſea, ſuſtains 60 feet of air, will here only ſuſtain 120 feet, and a 12th part of a line 10 feet.

Tho' the increaſe of each 12th of a line, adds M. *Mariotte*, may be found by the ſame rules whereby we find the logarithms; but in regard the ſum of geometrical progreſſions does not differ much from the ſum, found by taking thoſe progreſſion arithmetic proportions, I here make my *calculus* according to the latter proportion; and to find the ſum, take 7 and $\frac{1}{2}$, as an arithmetical mean between 5 and 10, which I multiply by 2016 12ths of lines, that is by 14 inches, the product 15120, or 2520 fathoms, will be the whole length of air, from a place of obſervation on a level with the ſea, to a place where the air is only $\frac{1}{2}$ that weight, that is, to a place where the mercury is only ſuſtained 14 inches high.

By the ſame method, M. *Mariotte* afterwards determines the reſt of the height of the atmoſphere; and to confirm this *calculus* of the height of the air, applies it to two celebrated obſervations, one whereof is delivered in M. *Paſcal*'s treatiſe abovementioned; and the other made ſome years ago, by M. *Caſſini*, which is as follows.

He took the height of a mountain in *Provence*, which is on the edge of the ſea, and found it 1070 feet; the mercury in the barometer which he uſed, ſtood at 28 inches at the foot of the mountain, and at the top thereof ſunk 16 lines $\frac{1}{3}$.

In examining this obſervation, M. *Mariotte* makes uſe of an arithmetical progreſſion, according to which, ſuppoſing that 63 feet height of air correſpond at a level with the ſea to 1 line of mercury, he finds that the height, where the mercury is to ſink 16 lines $\frac{1}{3}$, is 1080 feet, which comes very near the 1070 feet, obſerved by M. *Caſſini.*

As the arithmetical proportions uſed by M. *Mariotte*, in examining theſe obſervations, are not intirely conformable to the geometrical progreſſions, reſulting from his rule of the condenſation of the air, which however inconſiderable in little heights, may occaſion very ſenſible differences in great ones, I have formed a table upon M. *Mariotte*'s principals, wherein the height of air is expreſſed, which correſponds to each line's decreaſe of height of mercury, from the ſurface of the ſea upwards. In this table I have ſuppoſed with M. *Mariotte*, that the mercury is ſuſtained to the height of 28 inches on a level with the ſea, and that in this caſe 63 feet high of air correſponds to a line of mercury.

By this table it appears, that when the mercury ſinks 16 lines $\frac{1}{3}$, the height of air correſponding to the laſt line, is 66 feet, 1 inch, 9 lines, and the height of the place of obſervation from the ſurface of the ſea 176 fathom, 5 inches, 7 lines, or 1056 feet, 5 inches, 7 lines, which is leſs by 23 feet, than what M. *Mariotte* had determined by his arithmetical progreſſion. A proof that his manner of examining obſervations differs conſiderably from the principals he himſelf has eſtabliſhed, which will appear ſtill more viſibly in the obſervations I have to relate hereafter, which were made at much greater heights.

If in lieu of taking 63 feet for the height of air correſponding to a line of mercury on a level with the ſea, we ſuppoſe it 60 feet, as M. *Mariotte* does for determining the height of the atmoſphere, we ſhould have for the 16 lines $\frac{1}{3}$ decreaſe of the height of the mercury only 1005 feet height of air, which is much leſs than was found on the preceding ſuppoſition; but as it would be eaſy to reconcile this obſervation of my father with M. *Mariotte*'s rule, by only ſuppoſing the height of air at the ſurface of the ſea ſomewhat greater than what he has laid down, we proceed to examine the ſecond obſervation, which is that of M. *Paſcal*, and which he endeavours likewiſe to reconcile to his principals.

The other obſervation, ſays he, was made in a high mountain near the city *Clermont* in *Auvergne*, the principal circumſtances whereof were theſe.— The mercury in the barometer at the bottom of the mountain ſtood at 26 inches, 3 lines $\frac{1}{2}$; when carried 27 fathoms high, it ſunk to 26 inches, 1 line; at 150 fathoms, it ſunk to 25 inches; and laſtly, towards the top of the mountain, which was 500 fathoms above the firſt place, the mercury ſtood at 23 inches, 2 lines. The firſt obſervation ſhews us, that the loweſt place near *Clermont* is much higher than the vaults of the obſervatory, and conſequently that a line of mercury here muſt be equivalent to above 63 feet. It may be calculated thus:

The difference between 26 inches, 3 lines $\frac{1}{2}$, and 28 inches, is 20 lines $\frac{1}{2}$, and according to the *calculus* above declared, the laſt diviſion muſt increaſe it about 7 feet, above 63, for the produce of 63 by 21, divided by 168, gives ſomewhat above 7 feet, which added to 63, gives 70 feet; ſuppoſing therefore that the firſt line of mercury were then equivalent to 70 feet of air, reckoning from the loweſt place of obſervation, M. *Mariotte* finds the height of the place of the laſt obſervation to be 2940 feet, or 490 fathoms of air.

In examining this obſervation, M. *Mariotte* makes uſe of two arithmetical progreſſions: by the firſt, he finds that at a decreaſe in the height of mercury of 20 lines ½, the height of air correſponding to the laſt line of the mercury is 70 feet; whereas according to the table, it ſhould only be 67 feet; ſo that between the height of air correſponding to a line of mercury on a level with the ſea, and that ariſing from his *calculus*, when the mercury is ſunk 20 lines ½, he finds by his arithmetical progreſſion a difference of 7 feet inſtead of 4 feet, which ariſe by the geometrical progreſſion, ſo that the error makes it almoſt double.

He would eaſily have diſcovered theſe differences, if agreeably to his rule he had made, as to 28 inches height of mercury on a level with the ſea, are to 26 feet, 3 lines ½ height of mercury in the loweſt place at *Clermont*, ſo are 63 feet height of air on a level with the ſea to 67 feet, 1 inch height of air, which correſpond to a line of mercury in the loweſt part of *Clermont*.

The ſecond progreſſion which he uſes, afterwards carries him ſtill farther from the truth, but not to enter into too long detail, it may ſuffice to compare what ariſes from this progreſſion, with what is expreſſed in the table drawn upon his own principals; it will here appear, that as the height of mercury was 20 lines ½ below the ſtandard at *Clermont*, the height of that city above the ſurface of the ſea muſt be 222 fathoms, 2 feet, 1 line, or 1334 feet, that the mercury having ſunk 37 lines $\frac{1}{2}$ between *Clermont* and the top of *Pui de Dome*, which amounts to 58 lines in all from the ſurface of the ſea, the height of that mountain muſt be 670 fathom above the ſurface of the ſea; from whence ſubſtracting 222 fathoms, the height of *Clermont* above the ſame ſurface, we have 448 fathoms for the height of *Pui de Dome* above *Clermont*; whereas M. *Mariotte* had made it by his *calculus* 490 fathoms, and M. *Paſcal* 500 fathoms.

It will hereafter appear, that the height of *Pui de Dome* above *Clermont* is upwards of 500 fathoms, and conſequently differs ſtill farther from the 448 fathoms, which ariſe from the principals of M. *Mariotte.*

It may here be proper to enquire, whether the obſervations, ſince made by M. *de la Hire* on the mountain *Clairet* in *Provence*, and thoſe made by myſelf in my expedition for continuing the meridian, agree with the principals of M. *Mariotte.* ——M. *de la Hire* found upon the mountain *Clairet* the height of the mercury to be 26 inches, 4 lines $\frac{1}{2}$; and the like operation being repeated at the edge of the ſea 3 hours after, the mercury was found 28 inches, 2 lines high, the difference therefore is 1 inch, 9 lines $\frac{1}{2}$, and the height of the rock was found by menſuration to be 277 fathoms.

Now by the table calculated on M. *Mariotte's* principals, the height of air correſponding to 1 inch, 9 lines $\frac{1}{2}$, is 233 fathoms, 3 inches, which is leſs by 23 fathoms $\frac{1}{2}$, than what M. *de la Hire* found it by his obſervations.

One of the moſt acurate obſervations I made in continuing the meridian, was on the tower *de la Maſſana*, near *de Collioure.* The diſtance of this tower, from the place where we obſerved its height, was determined by the triangles of the meridian line. The height of the place of obſervation at *Collioure*, above the ſurface of the ſea, was meaſured very exactly with a cord, and the angle of height taken with a very good inſtrument, was above 7 degrees; ſo that an error of a minute in the obſervation would not have occaſioned an error of a fathom in the determination of the height.

This height was further confirmed by an obſervation made from the tower *de S. Elme*, whoſe height from the ſurface of the ſea, was exactly known by the firſt obſervation, we determined the height of this tower above the place of obſervation at *Collioure* to be 397 fathoms, and above the ſurface of the ſea 408 fathoms.

On the 12th of *March*, when the barometer at *Collioure* ſtood at 28 inches, we carried it to the foot of the tower *de la Maſſane*, where we found the mercury ſunk to 25 inches, 5 lines, the difference is 2 inches, 7 lines, which correſpond to 397 fathoms; looking in the table for the height of air correſponding to 2 inches, 7 lines, we find it 342 fathoms in lieu of 397 found by the obſervation.

Hence it appears, that the heights ariſing from the principal of M. *Mariotte*, do not agree with obſervation, but recede the further therefrom, as the diſtances are greater; for there is no room to aſcribe this difference of 55 fathoms, found between theſe heights, to any error that could have crept into the menſurations of the heights of the mountain and mercury, thoſe obſervations having been made with all the exactneſs imaginable.

We took the height of *Bugarache*, a mountain of *Languedoc* near the ſea-coaſt, in three different manners, and found it 648 fathoms.

The height of the mercury was here found on the 15th of *January* two hours after noon, to be 23 inches, 8 lines $\frac{1}{2}$. At *Paris*, it was found on the ſame day, at 7 in the morning, 27 inches, 3 lines $\frac{1}{4}$, and it ſunk in the courſe of the day about $\frac{1}{2}$ a line; ſo that we may ſuppoſe it 27 inches, 3 lines, and by adding thereto the 4 lines correſponding to the 40 fathoms height of the obſervatory above the ſurface of the ſea, we ſhall have the height of the mercury on a level with the ſea, 27 inches, 7 lines, which is more than what we found at *Bugarache*, by 3 inches, 10 lines $\frac{1}{2}$.

In the table, we find that the height of air correſponding to 3 inches, 10 lines $\frac{1}{2}$, is 527 fathoms, which is leſs by 121 fathoms than the 648 fathoms, found by the obſervation of the height of the mountain. Could the height of the mercury at the brink of the ſea, have been obſerved at the ſame time that we obſerved it on the mountain, nothing could have been further required for the exactneſs of the obſervation; but this we could not do becauſe otherwiſe employed.

The two moſt conſiderable obſervations after theſe, were thoſe of two mountains of *Auvergne*, near the mount *d'Or*; one whereof is called *la Coſte*, and the other *la Courland*. On the firſt of theſe, which is 851 fathoms above the ſurface of the ſea, we found the height of the mercury on the 9th of *October*, 3 hours after noon, to be 23 inches, 4 lines. At *Paris*, it was found at 5 in the evening, 27 inches, 10 lines, which is higher by 4 inches, 6 lines, than on the top of the mountain.

On the 12th of *October*, about noon, we find on the top of the *Courlande*, which is 838 fathoms above the ſea, the height of the mercury to be 23 inches, 4 lines. At *Paris*, it was 27 inches, 10 lines, which is higher by 4 inches, 6 lines, than on the top of the mountain, agreeably to what we found on the mountain *de la Coſte*. This difference ſhould have been ſomewhat leſs, by reaſon that the height of the *Courlande* is not ſo great as that of *la Coſte*; but there is no hopes of arriving at a greater exactneſs, it being impoſſible but ſome error muſt ariſe, both in the obſervation of the heights taken with inſtruments, and in thoſe of the barometer, obſerved in two different places; adding 4 lines, which correſpond to the height of the obſervatory, to 4 inches, 6 lines difference, between the heights of mercury found at the ſame time on theſe mountains and at the obſervatory, we ſhall have 4 inches, 10 lines for the difference of the height between the ſurface of the ſea and the top of theſe mountains, which we may ſuppoſe 844 fathoms, by taking a medium between the two obſervations.

For theſe 4 inches, 10 lines, the table gives us 669 fathoms high in lieu of 844 fathoms found by obſervation, which makes a difference of 175 fathoms. This is too conſiderable, to be owing to any error in the obſervations; for tho' the mountain be at a diſtance from the ſea, yet its height is found exactly enough, without any great number of operations.

It remains to examine the reſult of the obſervation made on the *Pui de Dome*, whoſe height above the ſurface of the ſea, was found 810 fathoms.

It were to be wiſhed, that while M. *Perier* obſerved the mercury on this mountain, and that of *Clermont*, the like had been done at *Paris*, whoſe height above the ſurface of the ſea is preciſely known; but this defect may be ſupplied by ſome obſervations of the greateſt and leaſt height of the mercury at *Paris* and *Clermont*, related in a letter of M. *Perier*, inſerted in the treatiſe of the *equilibrium* of fluids.

At *Clermont*, the higheſt was 26 inches, 11 lines ½, *Feb.* the 14th, 1651.

At *Paris*, the higheſt was 28 inches, 7 lines, *Nov.* the 3d and 5th, 1649.

The difference is 1 inch, 7 lines ½.

At *Clermont*, the loweſt was 25 inches, 8 lines, *Oct.* the 5th, 1649.

At *Paris*, the loweſt was 27 inches, 3 lines ½, *Oct.* the 4th, 1649.

The difference is 1 inch, 7 lines ½.

The difference between the greateſt height of the barometer at *Paris* and at *Clermont*, is 1 inch, 7 lines ½; the ſame which is found between the barometer at thoſe two places when the loweſt, which ſeems to intimate, that there is nearly the ſame conſtitution of air in both places.

If this difference therefore be ſuppoſed the ſame that correſponds to the difference of height between *Clermont* and *Paris*, and that the place of obſervation at *Paris* be 25 fathoms above the level of the ſea, which correſpond to 2 lines ½ of mercury, we ſhall have 1 inch, 10 lines of mercury, for the height of *Clermont* above the ſurface of the ſea.

Now for 1 inch, 10 lines, the table gives us 239 fathoms for the height of *Clermont* above the ſurface of the ſea, which ſubſtracted from 810 fathoms, the height of *Pui de Dome* above the ſurface of the ſea, leaves 571 fathoms for the height of *Pui de Dome* above *Clermont*, in lieu of 500 fathoms, which M. *Perier* ſuppoſes of 490 fathoms, which M. *Mariotte* reduces by his *calculus*,

culus, and of 448 which follow from his principles, all the obſervations hitherto related conſpire in making the dilatation of the air, as we recede from the earth, greater than what agrees with the principals of M. *Mariotte*, it even ſeems, as if in the two obſervations compared by him with his rules, he had been aware of this difficulty, and had been induced thereby to depart in ſome meaſure from his rules, and make uſe of an arithmetical progreſſion, which he ſuppoſes not to differ ſenſibly from the geometrical one; tho' we have ſhewn, that the difference is in reality very conſiderable.

The height of the air, which ariſes from M. *Mariotte*'s rules, deviating ſo far from the obſervations above related, 'tis no wonder, it ſhould not agree with what M. *Maraldi* has eſtabliſhed, which is founded on experiment, and exhibits all our obſervations with tolerable exactneſs; they may eaſily be compared together, the heights of air ariſing from M. *Mariotte*'s rule being placed in the table againſt thoſe ariſing from our obſervations, it will here appear, that at 5 inches fall of mercury, the height of air correſponding to a line of mercury, ſhould be 20 fathoms, which is rarer by $\frac{1}{2}$ than in a level with the ſea, in lieu of 12 fathoms, 4 inches, 8 lines, which ariſe from the rules of M. *Mariotte*, &c.

It will be difficult likewiſe to reconcile the conſequences flowing from his experiments, with his reaſonings.—For inſtance, firſt, that if luke-warm water be placed $\frac{5}{4}$ of a league high, it will boil; ſince in the receiver of an air-pump it boils vehemently, when $\frac{1}{2}$ the air is exhauſted. And, 2dly, that if there were a mountain a league and a half high, men, or even birds, could not live thereon, by reaſon their blood being only preſſed by half the weight of air, and being hotter than lüke-warm water, would yield abundance of air-bubbles, which would prevent its circulation, and diſturb the natural œconomy of the heart, and other parts.

For, according to our obſervations, the height of air correſponding to a line of mercury at 844 fathoms height, is 19 fathoms, 3 feet, which is ſomewhat leſs than double the height correſponding to a line of mercury on a level with the ſea, and yet we perceived no inconvenience from the rarefaction of the air, if we ſuppoſe the dilatation of the air to perſiſt in the *ratio* above-eſtabliſhed from experiment, the height of air correſponding to a line of mercury on the *Canigou*, which is 1450 fathoms, or ¼ of a league high above the ſurface of the ſea, will be found 24 fathoms; but tho' we were only to ſuppoſe it a little above 20 fathoms, this ſhould ſuffice to produce all the effects mentioned by M. *Mariotte*; and yet ſeveral perſons have been on the mountain, and a pyramid was even erected by his majeſty's command on the higheſt part of it, for the conveniency of our obſervations, yet we do not hear of any ſuch accident befalling them.

A table of the height of air which anſwers to the height of the quickſilver in the barometer.

Falling of the quickſilver.		The air's height anſwering to each line of quickſilver according to M. *Mariotte*.				The air's height upon the ſurface of the ſea according to M. *Mariotte*.				The air's height which anſwers to each line of quickſilver according to our obſervations.		The air's height upon the ſurface of the ſea according to our obſervations.		Height of the quickſilver.	
In.	*L.*	*T.*	*F.*	*In.*	*L.*	*T.*	*F.*	*In.*	*L.*	*T.*	*F.*	*T.*	*F.*	*In.*	*L.*
0	0	10	3	0	0	0	0	0	0	10	0	0	0	28	0
	1	10	3	2	3	10	3	2	3	10	1	10	1		11
	2	10	3	4	6	21	0	6	9	10	2	20	3		10
	3	10	3	6	10	31	4	1	7	10	3	31	0		9
	4	10	3	9	1	42	1	10	8	10	4	41	4		8
	5	10	3	11	4	52	5	10	0	10	5	52	3		7
	6	10	4	1	9	63	3	11	9	11	0	63	3		6
	7	10	4	4	1	74	2	3	10	11	1	74	4		5
	8	10	4	6	5	85	0	10	3	11	2	86	0		4
	9	10	4	8	10	95	5	7	1	11	3	97	3		3
	10	10	4	11	2	106	4	6	3	11	4	109	1		2
	11	10	5	1	7	117	3	7	10	11	5	121	0		1
1	0	10	5	4	0	128	2	11	10	12	0	133	0	27	0
	1	10	5	6	5	139	2	6	3	12	1	145	1		11
	2	10	5	8	10	150	2	3	1	12	2	157	3		10
	3	10	5	11	4	161	2	2	5	12	3	170	0		9
	4	11	0	1	9	172	2	4	2	12	4	182	4		8
	5	11	0	4	3	183	2	8	5	12	5	195	3		7
	6	11	0	6	9	194	3	3	2	13	0	208	3		6
	7	11	0	9	3	205	4	0	5	13	1	221	4		5
	8	11	0	11	10	216	5	0	3	13	2	235	0		4
	9	11	1	2	4	228	0	2	7	13	3	248	3		3
	10	11	1	4	11	239	1	7	6	13	4	262	1		2
	11	11	1	7	7	250	3	3	1	13	5	276	0		1
2	0	11	1	10	2	261	5	1	3	14	0	290	0	26	0
	1	11	2	0	9	273	1	2	0	14	1	304	1		11
	2	11	2	3	4	284	3	5	4	14	2	318	3		10
	3	11	2	6	0	295	5	11	4	14	3	333	0		9
	4	11	2	8	8	307	2	8	0	14	4	347	4		8
	5	11	2	11	4	318	5	7	4	14	5	362	3		7
	6	11	3	2	1	330	2	9	5	15	0	377	3		6
	7	11	3	4	10	342	0	2	3	15	1	392	4		5
	8	11	3	7	7	353	3	9	10	15	2	408	0		4
	9	11	3	10	4	365	1	8	2	15	3	423	3		3
	10	11	4	1	1	386	5	9	3	15	4	439	1		2
	11	11	4	3	11	388	4	1	2	15	5	455	0		1
3	0	11	4	6	9	400	2	7	11	16	0	471	0	25	0

Falling of the quickſilver.		The height of the air anſwering to each line of the quickſil-ver according to M. *Mariotte.*				The height of the air upon the ſurface of the ſea according to M. *Mariotte.*				The height of the air which anſwers to each line of quick-ſilver according to our obſervations.		The height of the air upon the ſurface of the ſea according to our obſervations.		Height of the quickſilver.	
In.	*L.*	*T.*	*F.*	*In.*	*L.*	*T.*	*F.*	*In.*	*L.*	*T.*	*F.*	*T.*	*F.*	*In.*	*L.*
3	0	11	4	6	9	400	2	7	11	16	0	471	0	25	0
	1	11	4	9	6	412	1	5	5	16	1	487	1		11
	2	11	5	0	4	424	0	5	9	16	2	503	3		10
	3	11	5	3	3	435	5	9	0	16	3	520	0		9
	4	11	5	6	2	447	5	3	2	16	4	536	4		8
	5	11	5	9	1	459	5	0	3	16	5	553	3		7
	6	12	0	0	0	471	5	0	3	17	0	570	3		6
	7	12	0	2	11	483	5	3	2	17	1	587	4		5
	8	12	0	5	11	495	5	9	1	17	2	605	0		4
	9	12	0	8	11	508	0	6	0	17	3	622	3		3
	10	12	0	11	11	520	1	5	11	17	4	640	1		2
	11	12	1	2	11	532	2	8	10	17	5	658	0		1
4	0	12	1	6	0	544	4	2	10	18	0	676	0	24	0
	1	12	1	9	1	556	5	11	11	18	1	694	1		11
	2	12	2	0	2	569	2	0	1	18	2	712	3		10
	3	12	2	3	3	581	4	3	4	18	3	731	0		9
	4	12	2	6	5	594	0	9	9	18	4	749	4		8
	5	12	2	9	7	606	3	7	4	18	5	768	3		7
	6	12	3	0	9	619	0	8	1	19	0	787	3		6
	7	12	3	3	11	631	4	0	0	19	1	806	4		5
	8	12	3	7	2	644	1	7	2	19	2	826	0		4
	9	12	3	10	5	655	5	5	7	19	3	845	3		3
	10	12	4	1	9	669	3	7	4	19	4	865	1		2
	11	12	4	5	0	682	2	0	4	19	5	885	0		1
5	0	12	4	8	4	695	0	8	8	20	0	905	0	23	0
	1	12	4	11	8	707	5	8	4	20	1	925	1		11
	2	12	5	3	0	720	4	11	4	20	2	945	3		10
	3	12	5	6	5	733	4	5	9	20	3	966	0		9
	4	12	5	9	10	746	4	3	7	20	4	986	4		8
	5	13	0	1	4	759	4	4	11	20	5	1007	3		7
	6	13	0	4	9	772	4	9	8	21	0	1028	3		6
	7	13	0	8	3	785	5	5	11	21	1	1049	4		5
	8	13	0	11	10	799	0	5	9	21	2	1071	0		4
	9	13	1	3	4	812	1	9	5	21	3	1092	3		3
	10	13	1	6	11	825	3	4	0	21	4	1114	1		2
	11	13	1	10	6	838	5	2	6	21	5	1136	0		1
6	0	13	2	2	2	852	1	4	8	22	0	1158	0	22	0
	1	13	2	5	10	865	3	10	9	22	1	1180	1		11
	2	13	2	9	6	879	0	8	0	22	2	1202	3		10
	3	13	3	1	3	892	3	9	3	22	3	1225	0		9
	4	13	3	5	2	906	1	2	5	22	4	1247	4		8
	5	13	3	8	9	919	4	11	2	22	5	1270	3		7
	6	13	4	0	7	933	2	11	9	23	0	1293	3		6
	7	13	4	4	5	947	1	4	2	23	1	1316	4		5
	8	13	4	8	3	961	0	0	5	23	2	1340	0		4
	9	13	5	0	1	974	5	0	6	23	3	1363	3		3
	10	13	5	4	0	988	4	4	6	23	4	1387	1		2
	11	13	5	8	0	1002	4	0	6	23	5	1411	0		1
7	0	14	0	0	0	1016	4	0	6	24	0	1435	0	21	0
	1	14	0	4	0	1030	4	4	6	24	1	1459	1		
	2	14	0	8	0	1044	5	0	6	24	2	1483	3		

VI. *That the experiments, uſed to prove that fluids are firſt condenſed and cooled before they are dilated, at the approach of heat, do not prove it; and that this apparent condenſation is merely owing to the dilatation of the glaſs and of the veſſels, which contain theſe fluids, by M.* Amontons [*]; *tranſlated by Mr.* Chambers.

Tho' reaſonings, founded on experiment, ſhould always be the ſureſt and moſt juſt, yet we are frequently betrayed into errors by means thereof. At a meeting of the academy on the 12th of laſt *November*, I ſhewed, that a glaſs bottle, terminated with a narrow neck or tube, being filled with water to about ¼ the height of the tube, and the hands applied thereon, the heat of the hand ſunk the water in the tube before it made it riſe.

And M. *Geoffroy*, in a memoir upon cold diſſolutions, relates a parallel fact; putting, ſays he, ſome cold water in a large baſon, I plunged a glaſs cucurbit full of equally cold water in the middle thereof, and put a very active thermometer in the cucurbit, then caſting 4 or 5 ſhovels of lighted coals into the baſon, the liquor in the thermometer inſtantly ſunk 2 or 3 lines, but roſe again in a few moments, *&c.*

In my phyſical remarks and experiments, *&c.* printed in 1694, ſpeaking of 2 thermometers, one whereof was full of *aqua ſecunda*, and the other of ſpirit of wine; I obſerved, that having applied my hand upon the former thermometer, I found it firſt ſink upwards of a line, after which it roſe again very conſiderably, while the other thermometer, which I held in the other hand, aroſe at once, without any other fall of its liquor at all.

---

[*] Mar. 18, 1705.

But before any of this, *Borelli* and *Iſaac Voſſius*, the firſt, in his treatiſe of percuſſion, and the other, in his treatiſe of the motion of the winds and ſea, mention the like experiments; *fiat*, ſays *Borelli* *, *phiala vitrea* A B C, *ejuſque fiſtula tenuiſſima* A B, *impleaturque aqua vel quolibet alio fluido uſque ad terminum* D: *ſi poſtea eadem phiala immergatur intra vas* E F G H, *aqua calida plenum ſubito aqua deprimitur a ſigno* D *uſque ad* O, *& e contra ſi immergatur intra aquam glacialem ſubito aqua ſublevatur uſque ad ſignum* I. As to *Iſaac Voſſius*, his way of repreſenting the experiment is thus: Take a glaſs bottle with a broad belly and a narrow neck, fill it full of cold water, and plunge it in hot or barely lukewarm water; after the firſt ſhrinking, which only holds a moment, and which, upon the ſudden contact of the hot water, makes the cold ſink a little, it quickly riſes again, but if you heat the water in the bottle ever ſo little, and plunge it in cold water, the contrary will be found.

Now tho' each of theſe experiments have ſomething peculiar to them, which ſhews, that they had all been made ſeparately, yet they agree in this, that the liquor ſinks firſt before it riſes, upon the approach of heat, which could not be, unleſs either the capacity of the bottle were increaſed, or the liquor contained in it condenſed, or both the one and the other, which has given riſe to two different opinions.

---

* Plate V. Fig. 3.

Voſſius and M. *Geoffroy*, hold for the condenſation of the liquor, and *Borelli*, to whom I likewiſe adhere, for the dilatation of the glaſs; in effect, the former ſyſtem, which appears only like a paradox, will perhaps be found at bottom a meer paralogiſm, it being utterly inconceivable how heat ſhould condenſe a liquor, which makes ſo obſtinate a reſiſtance againſt all compreſſion, as common water is known to do; the moſt plauſible way of ſolving it would be, by alledging, that the fiery particles, diffuſed thro' all bodies, both ſolid and fluid, have a tendency to re-unite, where they happen to be, in the greateſt quantity, which makes them relinquiſh for a little time, thoſe places where they are fewer—— but till there be ſome experiment to ſupport this reaſoning, it will have no great force.

By the way, it being of the laſt importance, if we would extend our knowledge, not to admit any falſe principal, and there being but too much biaſs in our nature, to what ſeems ſurpriſing, it may not be amiſs to make a ſtrict ſcrutiny of theſe opinions, in order whereto I argue thus:

If the ſeeming condenſation of the fluid, upon the approach of heat be real, its effect will be more ſenſible, the more ſuſceptible of condenſation the liquor made uſe of is; and on the contrary, if it be the bottle that inlarges its capacity, the effect will be the leſs ſenſible, as the liquor made uſe of is more liable to be condenſed, inaſmuch as it cannot have this quality, without at the ſame time having its oppoſite one, *viz.* rarefaction, which of courſe will undo the effect of the augmentation of the bottle more readily, than a fluid which rarefies with leſs eaſe; and this is what we actually find; for in the experiment above related of the two thermometers, one filled with *aqua ſecunda*, and the other with ſpirit of wine; tho' my two hands were heated as equally as poſſible, I perceived no condenſation in the ſpirit of wine before it roſe, tho' there was a very ſenſible one in the *aqua ſecunda*; and this, notwithſtanding that the bowl of the former was 12 times leſs than that of the latter, where, if the liquor had really condenſed upon the approach of heat, ſo ſmall a body muſt certainly have felt the impreſſion ſooner than a greater one; for notwithſtanding its ſmallneſs, its dilatation was 6 times greater than the large one, ſo that there could be no reaſon to hinder the ſpirit of wine from condenſing more conſiderably than the *aqua ſecunda*, provided the condenſation had really taken place; and hence we may ſafely infer, that 'tis the dilatation of the glaſs, which by increaſing the capacity of the bowls, produces this appearance of condenſation in the fluid, and that it can no longer be urged, as is done by *Voſſius*, that heat condenſes fluids, before it dilates them, any more than that ſuch fluids are colder at this time, than before the heat came near them; an opinion which has nothing in nature to keep it in countenance.

Tho' this experiment might have ſufficed to ſhew, that thoſe already related, do not prove the cooling or condenſation of fluids, upon the approach of heat, I have further confirmed it by another.

Thruſting the glaſs tube * A B, thro' the cork C, which ſtops a bottle D E, about 3 inches in diameter, and 4 inches high, almoſt to the bottom of the bottle, till the tube dipped in a little water depoſited therein, while the reſt of the bottle was only full of air, which raiſed the water of the tube to F 2 or 3 inches above the neck of the bottle.

---

* Fig. 4.

Now every body knows, that air readily receives the impreſſion of cold, and that we have no nicer thermometers than thoſe made in this manner, and yet applying both my hands upon the bottle, the water in the tube did not ſink above 2 or 3 lines, and even upon repeating the experiment ſeveral more times, it did not ſink at all, but roſe quickly to the top of the tube, whereas, when the bottle is quite full of water, the deſcent of the water in the tube A B, by the warmth of the hand, is upwards of ½ an inch. I might eaſily have repeated theſe experiments, with other degrees of heat, more conſiderable than thoſe of the hand, but this ſeems unneceſſary.

E're we conclude, it may be obſerved, that by theſe words of *Borelli*, *impleaturque aqua vel quolibet alio fluido*, it appears, that tho' he really aſcribed the ſinking of the water to the dilatation of the bottle, he had conſidered the different ſenſibility of liquors, tho' this difference of ſenſibility be the only proof of the dilatation of glaſs.

VII. *Obſervations on the declination of the needle, made in a voyage from* France *to the* Eaſt-Indies, *and in the return from the* Indies *to* France, *in the years* 1703 *and* 1704, *by M.* Caſſini *the ſon* *.

M. *Gualtieri*, the pope's nuncio in ordinary, communicated to us lately two charts, which were given him by the chevalier *de Fontenay*, who commanded the veſſels *le Maurepas*, and *le Pondichery*, which carried the pope's legate to the *Indies*. In theſe charts are deſcribed the courſe, they went day by day from *Port-Louis*, whence they ſailed *April* 23, 1703, to *Malacca*, and from *Malacca* to *Port-Louis*, where they arrived in *Auguſt*, 1704. On one of theſe charts is deſcribed the variation of the needle, obſerved not only in the voyage from *France* to the *Indies*, but alſo in the return; and there is a much more conſiderable number of them, than M. *de May* has deſcribed on his chart, the relation of which we have already made to the academy.

---

* March 28, 1705.

Among theſe obſervations, there are many which were made on the ſame days with thoſe of M. *de May*. There are alſo ſome in M. *de May*'s chart, which are not deſcribed on the new chart; ſo that it appears they have been made by different obſervers, and perhaps even in two different ſhips, thoſe which were made on the ſame day, not agreeing all of them exactly, having ſome difference in ſome of them, which neverthleſs does not exceed more than half a degree. That agreement which we have already found between the variations obſerved by M. *de May*, and thoſe remarked by Dr. *Halley*, in his chart, has induced us to examine thoſe which we have ſince received; and we have placed on Dr. *Halley*'s chart, all the obſervations which are remarked on theſe new courſes, which are to the number of 94.

To place them with the greateſt exactneſs poſſibly, regard has been had to the longitude, which is deſcribed in theſe different charts. In the chevalier de *Fontenay*'s chart, the firſt meridian paſſes through *Cape-Verd.* The longitude of the *Cape of Good-Hope,* is there ſet down at 38°; that of the iſle of *Bourbon* or *Maſcaregne,* where they landed going and coming, at 76°; of *Pondichery,* 102°, 30′; and of *Malacca,* 122°.

In Dr. *Halley*'s chart, which takes the longitudes from the meridian of *London,* the difference between the longitude of *Cape-Verd,* and the *Cape of Good-Hope,* is there ſaid to be 33° ½, which is 4° ½ leſs than in the new chart. The difference which is between the *Cape of Good-Hope* and the iſle of *Bourbon,* 38°; between the iſle of *Bourbon* and *Pondichery,* 25°, 40′; and between *Pondichery* and *Malacca,* 23°.

The difference of longitude between *Cape-Verd* and the *Cape of Good-Hope,* being not the ſame in theſe two charts, the neceſſary reduction has been made, to place on Dr. *Halley*'s chart, thoſe places where the declination was obſerved in the new chart, which are compriſed between the meridians of theſe two capes. As to the other differences which are very near the ſame, there was no need of making any conſiderable reductions in them.

It is ſeen by comparing theſe obſervations, that there are many of them which give preciſely the ſame declination of the needle, as is deſcribed in the chart of variations; and that the greateſt part of them are not more than a degree diſtant from it. Some of them differ more conſiderably, principally thoſe which were obſerved in the return, from 106°, 50′ longitude; and 5° ſouth latitude, to 81°, 30′ longitude, and 20°, 30′ ſouth latitude. Theſe obſervations differ from thoſe deſcribed in the chart of variations, from 3 to 8 degrees, and do not even agree with thoſe which were made in the voyage from *France* to the *Indies*, which are a little more ſouthward. Thus it may be conjectured, that there was ſome particular cauſe, which produced theſe differences.

I have ſaid in the preceding memoir, that if on examining the obſervations of the variation of the needle made in ſeveral other courſes, there is found a like conformity with that which I have related, ſome uſe might alſo be made of it, in determining the longitudes, more eſpecially in thoſe ſeas which are beyond the equator, where the lines which mark the variations, cut the parallels more perpendicularly. This is confirmed by ſome obſervations, which *F. Noël* has communicated to us within theſe few days. This is what he relates.

" Sailing in the *Indies*, in the year 1684, I " perceived by ſeveral pilots journals, and by " the converſations which I had with them, that " the variation of the needle has certain bounds " and fixed rules, at leaſt in regard to certain " places of the earth; ſo that when it is come " to certain degrees N. E. or N. W. it returns " towards the N. and never goes through the " whole circle; ſo that formerly it was N. E. " in ſome places, where it is now N. W. The " annual difference of this variation, by com" paring the journals of many years, was found " to be from 0, 9′, 30″. In the year 1702, when " I returned from *China* to the *Cape of Good-* " *Hope*, the needle declined 12°, 30′, from N. " to W. At 100 leagues from this cape towards " the *Indies*, it was 15°. At the point of the " iſle of *Madagaſcar*, it was 27°, which was " much greater than when I firſt paſſed by there, " going to the *Indies*. It keeps this rule pretty

“ certainly, from the port of *Liſbon* to the *Indies*, ſo that pilots, by inſpecting the needle's variation, know certainly at what longitude from land, and at what place they are. It is now fixed in the middle of the paſſage, between the *Braſils* and *Africa*, that is, it neither declines to the E. nor W. It muſt be obſerved, that ſometimes the needle loſes its virtue by length of time, and through the bad temperature of the air.

VIII. *New obſervations on the load-ſtone and on the magnetical needle, by M.* de la Hire, *junior* *; *tranſlated by Mr.* Chambers.

I do not here propoſe to give a new ſyſtem of the load-ſtone, nor to enumerate, nor to relate the ſeveral virtues of that ſtone; or the effects obſerved in ſteel, or iron touched thereby, but ſhall only endeavour to clear up ſome difficulties which occur in obſervations made with a needle; and ſubjoin ſome remarks upon the nature of the load-ſtone itſelf, and its compariſon with our globe of earth; which, by all the experiments hitherto made, appears to be a real load-ſtone.

'Tis known, that all obſervations of the variation of the needle made at ſea, are liable to many errors, on account of the great quantity of iron in all veſſels, which, according to its ſeveral poſitions, will draw the needle out of its true direction, not to mention the uſual conſtruction of this needle, or compaſs, as 'tis called, which is too groſs to give the declination very exact; but the obſervations, which we now make at land with large needles, very delicately hung, as that of 8 inches long, which we have uſed from the year 1682, after firſt determining a meridian plane with all the juſtneſs imaginable, and far from all ferruginous matter to apply the ſide of the box to have aſſured us of the juſt declination of the needle, and of its progreſſion, or changing, called its variation, as may appear from ſeveral memoirs, which we have already publiſhed.

---

* April 22. 1705.

Some philoſophers have imagined, not without a ſhew of reaſon, that the needles touched with different ſtones, do not exhibit different declinations, on account of the varieties obſervable in different needles at the ſame place, yet that ſuch inequalities might ariſe from the different ſtructure of the needles.

For needles touched with a ſtone, have only received from it a diſpoſition in their pores, to let the magnetic matter, circulating in a certain direction round the earth, paſs through them, after the ſame manner as the load-ſtones themſelves received this property at their firſt formation. So that the different load-ſtones cannot give different virtues to the needles, which can receive no direction, but according to the courſe of the magnetic matter, which being the ſame in the ſame place of the earth, muſt give them the ſame direction with its own; but tho' the magnetic matter act equally, and in the ſame direction in the ſame place, it may be diverted therefrom, and this differently, according to the different diſpoſition and figure of the bodies which receive it, as we find befel two load-ſtones, or two needles, when placed at liberty near each other out of the line of direction.

This made it be imagined, that needles, which have two pieces of ſteel at their ends joined together by a ſlender thread, might be conſidered as two load-ſtones, of different ſtrength and figure, placed apart from each other, but connected by ſome intermediate body; and if the two pieces of ſteel were of ſuch nature or figure, as that the magnetic matter ſhould have a different direction in the one from what it has in the other, and that one of them had received a ſtronger impreſſion from the touch than the other, it will neceſſarily follow, that the needle muſt take an intermediate direction compounded of the two, and different from that of the magnetic vortex of the earth; conſequently ſuch needles muſt exhibit different declinations from each other, as well as from needles of a different make.

Needles made broad in the middle, when terminating in points, may be ſuppoſed leſs liable to ſuch irregularities than thoſe thus furniſhed with pieces of ſteel at their ends; but yet they will not be entirely exempt therefrom, by reaſon of the inequalities of their matter and figure, which can never be perfect.

To aſcertain this point, we procured 4 needles to be made bigger at the middle than towards the ends, and terminating in a very fine point, they were each of them 8 inches long, two of them being as ſtrait and equable as poſſible, a third crooked like an S, and the fourth in ſhape of a bow; one of the ſtrait, and the two crooked ones, were touched with an excellent ſtone, which weighs 7 *lb.* and is ſtrong enough to turn a needle 6 or 7 feet diſtance; ſo that it muſt have a ſenſible *vortex* round it, above 12 feet in diameter; the other needle was touched with a very good ſtone belonging to M. *Butterfield.*

We proceeded next to examine the box, which was of an oblong figure, in order to be ſatisfied whether its ſides were parallel to each other, and to the line which paſſes through the pivot, and the two firſt points of diviſion of the two arches, which ſerve to meaſure the quantity of the declination, with regard to the point of the pivot; and the whole being thus rectified, we found by ſeveral obſervations, that the two ſtrait needles, and that bent like an S, had their points and the bottom of their cap, which plays on the pivot, perfectly in a right line; as to the other arched one, we found one of its extremities deviate from the right line 2°— 20′.

This done, we put the ſtrait needle which had been touched with our ſtone, and which is the needle we commonly uſe to take the declination by in the box, and placing the ſide of the box againſt the meridional plane, the needle gave us 9°, 25′, declination weſtwards, which agrees with our other obſervations; then we put the other ſtrait needle in the box, which had been touched with M. *Butterfield*'s ſtone, and found it give exactly the ſame declination 9°—25′—; yet another larger needle than either of them, which had two pieces of ſteel at the ends, and which had been touched with the ſame ſtone, had given us the declination 9°——52′— in the ſame place ſome time before; laſtly, the needle bent like an S, only gave it 8°——45′; and the laſt, which was arched ſtill leſs, *viz.* 8°——22′—.

Hence we may conclude, that needles touched with different ſtones, do not give different declinations; and that if there be any difference, it muſt either ariſe from the unequal hetrogeneous matter, or from the figure of the needle.

This is confirmed by the obſervations of the two ſtrait needles; as to that ſhaped like an S, its two halves being placed aſide of the right line which paſſes through its extremities, the point, which looked towards the north, only ſhewed 8°——45′— in lieu of 9°——25′, which might proceed from the compoſition of the direction of the magnetic matter in the two parts of the needle, and perhaps alſo in ſome meaſure from the matter thereof. As to that ſhaped archwiſe, a right line paſſing thro' its two points, would have given us 9°——32′—— declination, which deviates but little from that given by the ſtrait ones.

On the inequalities in the variation of the load-ſtone.

We ſhall not here relate the accounts of ſeveral authors touching the different declinations of the needle, the certainty of whoſe obſervations might be called in queſtion, but only give thoſe which we ourſelves have made, and compare them with ſome others, whereof we may be fully ſatisfied.

M. *Picard* informs us in his treatiſe of the meaſure of the earth, that in the year 1670, he had found the variation at *Paris* 1°——30′—— weſtwards with a needle, 5 inches long. That in the year 1666, there was no ſenſible declination at all by the ſame needle; but that in 1664, it declined 40′— eaſtward, the change having been 20′—— for each year. —— In the manuſcript of the ſame obſervations of M. *Picard*, we find in 1680 the declination of the ſame needle was 2°——40′—; conſequently from 1670 to 1680, the declination had only increaſed 1°——10′, which only gives 7′—— for the yearly increaſe.

From that time we have made a greater number of obſervations at the royal obſervatory, with a needle 8 inches long already mentioned, I ſhall here only relate ſome of the principal ones.

In 1683, *March* the 10th the declination was 3°——50′—— weſtwards.

In 1684 the needle declined 4°——10′——
In 1685 the declination ſtill remained 4°——10′——
In 1686 it was 4°——30′——
In 1692 it declined 5°——50′——
In 1693 —— 6°——20′——
In 1696 —— 7°—— 8′——
In 1698 —— 7°——40′——
In 1700 —— 8°——12′——
In 1701 —— 8°——25′——
In 1704 —— 9°——20′——

By conſidering theſe obſervations it appears, that the declination does not increaſe equably, that ſometimes it ſeems the ſame for two years together, but afterwards increaſes faſter than it ſhould have done. Without entering therefore into the reaſons of thoſe little variations, we ſhall content ourſelves to compare the diſtant obſervations together, in order to deduce the law of the variation therefrom, there being no reaſon to imagine, that from the time of its turning towards the weſt, it has either increaſed or ſlackened to the preſent time. Now ſetting aſide M. *Picard*'s obſervation of 1680, we ſhall find, that in 38 years, *viz.* from 1666, to 1704, the declination has increaſed 9°, 20′, which only gives about 14′ $\frac{1}{3}$ for each year.

It appears alſo from ſome ancient obſervations, that in the year 1580, the declination at *Paris* was 11°, 30′, eaſtward, which being compared with the obſervation of 1666, when there was no declination at all, gives ſomewhat leſs than 8′ — *per* year; whence one would imagine, that the variation had not been ſo great in thoſe days as now. Indeed, it may be obſerved by the way, that 'tis not very eaſy to meaſure and eſtimate minutes exactly, on a little circle of 8 inches diameter, and that the magnetic matter of the *vortex* of the earth might not be powerful enough to bring a larger needle exactly to the ſame point; ſo that 'tis no wonder, if we ſometimes find conſiderable diſtances between one year and another.

In a *Spaniſh* book, entitled the *Naval Theatre*, by *Don Franciſco de Seylas* and *Louera*, we find two cauſes aſſigned for the variation of the declination; one is the ſeveral mines of load-ſtones, found in the ſeveral parts of the earth; and the other the nature of the load-ſtones, wherewith the needles are touched.——As to the former, there is no doubt but large rocks of load-ſtone may affect the needles when near them; but that they ſhould have an effect at a great diſtance can hardly be conceived.

As to the latter, the author builds on ſome experiments, which he made in a mine of load-ſtones in the province of *Honduras*, in *America*. This mine, he ſays, conſiſted of two principal veins; one whereof was extended north and ſouth, and the other eaſt and weſt. In the former vein, he found a ſlip about two fingers broad, which was excellent load-ſtone, and placing a needle along this ſlip, he found it had no declination, but when the ſame needle was placed on the other vein, it had a ſenſible declination on both ſides; he adds, that he learnt hereby, that the north and ſouth vein predominated over the other.

Now what he ſays is highly probable, but it does not follow hence, that when the ſtones are taken out of the mine, and needles touched with them, ſuch needles muſt have the ſame direction, as the ſtone had in the mine; ſince in reality the needle does not take its direction from the ſtone, but only from the magnetic vortex of the earth; for otherwiſe, if the point of a needle were touched with that ſide of a ſtone, which looks to the eaſt or weſt in a free ſituation, it would follow, that this point of the needle would turn to the eaſt or weſt, which is contrary to all experience.

He adds farther, that having melted ſome of this vein of load-ſtone, he procured iron from it, which had the ſame virtue as the ſtone itſelf; and yet 'tis well known, that a load-ſtone, when heated only red-hot looſes all its virtue, and much more muſt it do ſo, when it has undergone a fuſion. What further diſcredits his whole account is, that he aſſures us, that putting two little pieces of this iron on the ends of a needle, it never varied either at land or ſea, a circumſtance by no means probable, and contradictory in ſome meaſure to his own relation. For what is now become of his rocks and mines of load-ſtone, which were laid down as the general cauſe of the needles varying.

Of the converſion of iron into load-ſtone.

If all the difference between load-ſtone and animated iron conſiſt in this, that the former is of a ſtony nature, and capable of being broke and reduced into a fine duſt; whereas the latter is incapable of both, by reaſon its parts are ſofter and more tenacious. 'Tis certain, that ruſty iron, if it be poſſeſſed of a magnetic power, muſt be conſidered as real load-ſtone, ſince iron in this ſtate ſeems to retain nothing of the metalline nature, but rather appears as a ſtone being eaſy to be broke and ground to duſt.

Gaſſendus, in the life of *Fab. de Peireſk*, relates, that the croſs on the ſteeple of St. *John d'Aix*, in *Provence*, having been thrown down by thunder, it was obſerved, that a cruſt of ruſt, which had gathered on the part of the iron of the croſs inſerted into the wall, was found very magnetical, though it had nothing remaining of the former quality of iron. This led ſome curious perſons at *Chartres*, a few years ago, to examine whether the ruſt on the bars of iron that bound the ſtones of one of the ſteeples of *Notredame*, when they were obliged to rebuild it, were not alſo converted into load-ſtone, and after examining ſeveral pieces thereof, they found ſome which were really pure load-ſtones, having nothing of the iron remaining, while others had no ſenſible virtue; and others very little. Several of theſe magnets I have ſtill by me.

On this occaſion my father ſearched among a multitude of pieces of iron ruſt, ſome of them very thick, which had been gathered from antient buildings, but could not find one that appeared magnetical; this is eaſily diſcovered by preſenting them gently to the point of an animated needle; for if they have acquired any power, they will attract this point on the one ſide, and repel it on the other.

Upon this, he bethought of a method of making this kind of load-ſtone with iron, not being able to attribute this converſion of iron into load-ſtone, but to two cauſes; the one the diſpoſition of the iron in the air with regard to the magnetic vortex of the earth, which might have given it a magnetic power, which it ſtill in ſome meaſure retained, tho' changed into ruſt or ſtone; the other to a particular property in ſome iron which diſpoſes it to be converted into load-ſtone. ———

To this purpoſe, he took one of the ſquare ſtones of *St. Leu*, and ſawing it to an angle of 60°—— with the horizon, placed it in the air in the direction of the meridian line, making withal ſeveral grooves in the flat ſide, and inſerting pieces of iron wiar into the ſame, according to the direction of the magnetic matter around the earth, with regard to our horizon. This was done in 1695, and the part of the ſtone, wherein the wiars were put, was covered over with the reſt which had been cut off of theſe wiars. Some were animated, and others not; they were placed about the diſtance of two inches from each other.

The deſign of all this *apparatus*, was only to learn, whether when the iron wiar ſhould in length of time be conſumed, the ruſt ariſing from it would not be a magnetical matter, and whether there would be any difference between the iron which had been touched, and that which had not.——He choſe the ſtone of St. *Leu*, as being that whereof the ſteeple of *Notredame* at *Chartres* had been built.

In fine, after the ſpace of 10 years, we found that only ſome of the iron wiars were quite turned into ruſt, tho' none of them were above ½ a line in diameter, but all the wiars, whether wholly or partly ruſted, had a ſtrong magnetic power, as appeared by preſenting them to a needle; and thoſe which had not been touched as much as the reſt, which can only be ſuppoſed owing to the length of time which they had continued in a proper poſition to receive the effect of the magnetic vortex of the earth, and to their being converted either wholly or partly into ſtone. The pieces of wiar were 4 or 5 inches long, and were held horizontally when preſented to the needle, to prevent their becoming animated by the vortex of the earth. Upon the whole, ſuch as were intirely changed into ruſt, were real load-ſtones; and the like might be ſaid of thoſe little flakes of ruſt, which came out of the reſt; tho' as theſe flakes would not hang to the extremity of the iron-wiar, which was not magnetical, but hung vigorouſly to the point of an animated knife, one would imagine, that they were not changed into load-ſtone, but that they ſtill retained ſomething of the iron, unleſs we chuſe to ſuppoſe, that ſuch ſmall particles of load-ſtone were not ſtrong enough, with regard to their weight, to ſuſtain themſelves upon iron, which had not been animated.

It cannot indeed be abſolutely ſaid, that ruſt retains none of the properties of iron, ſince we have found that ſome large pieces of ruſt, which made no impreſſion on a magnetical needle, ſupported by its pivot, when reduced to duſt, would ſtill hang to the point of an animated knife.

But theſe pieces of ruſt, which have no magnetic virtue of their own, are likewiſe incapable of receiving any from the touch of the beſt ſtone, it may therefore be imagined, that in this ruſt, which is about $\frac{1}{4}$ of an inch thick, and in all reſpects like good load-ſtone, the particles of iron, ſtill remaining, are too much fettered with the other matters mixed therewith, to be in a condition to receive the magnetic virtue of the earth's *vortex*; doubtleſs there is a good quantity of iron in the load-ſtones, which are real ſtones, ſince we can procure iron from them by the fire; but I hardly believe it poſſible to procure iron out of that which has been conſumed by ruſt.

This experiment led us to make another; we took ſome of thoſe little pieces of burnt and melted iron, which fall in ſparks and ſcales around the ſmith's anvil, and reduced them like a ſtone into a fine powder, which powder would ſtick very ſtrongly to the point of an animated knife. But what was more, ſome of thoſe pieces which had been melted, and were capable of being reduced to powder, were ſtill ſuſceptible of the magnetic power; and when touched with a good ſtone, took up a large quantity of ſteel-filings.

By this it appears, that the fire in melting and conſuming iron, does not intirely deſtroy its nature, tho' it only leaves it in the appearance of a ſtone. There is little or no iron ore but is a load-ſtone, as may be eaſily known, by preſenting it on ſeveral ſides to a magnetic needle; but tho' they have the character of real load-ſtone, they are ſometimes ſcarce able to ſuſtain the ſmalleſt filings.

I have had a large load-ſtone for ſome years, which weighs a 100 *lb.* and tho' it be not of the beſt matter, it appears tolerably good by its effects, ſince it will turn a needle at 6½ feet diſtance, and conſequently muſt have a ſphere around it of 13 feet diameter. This ſtone we have rounded in ſome meaſure, and filled up the greater inequalities with a plaiſter of the ſame colour as the ſtone, ſo that the whole makes a kind of ball, about a foot in diameter.

Having ſearched for the poles of this ſtone, we found them in two points diametrically oppoſite, upon which we drew an equator in the middle between them, and divided it from 30°, to 30°; thro' which we drew meridians, in order to obſerve the ſeveral declinations of the needle with the more exactneſs; we alſo noted its declination in all the points where the meridians cut the equator; by which it appears, that in a certain ſpace it is weſtern, in another eaſtern, and in ſeveral places 0; the greateſt of theſe declinations is 26°, and three of the places which are no declination, are in the artic circle. In effect, by tracing the ſeveral points wherein the needle had no declination, we formed two different lines, one whereof began from the artic circle, and returned to it again, by a meridian, after deſcending about 10° beyond the equator, and deſcribing a ſpace about 110°, parallel to that circle; the other beginning likewiſe in the pole or circle, makes ſeveral turnings near the ſame; after which it takes its courſe north and ſouth, and making ſome more inflexions, cuts the equator, and terminates in the antartic circle. All which declination, and the lines where there are none, bear a near relation to what we find on the terreſtrial globe.

By the ſeveral experiments hitherto related, it appears, that the different declinations of the load-ſtone, obſerved on the terreſtrial globe, are only owing to the different diſpoſition of the magnetic matter in the earth, as we judge, to be the caſe in our globular magnet, for there is no doubt but the vortex of magnetic matter, was the firſt cauſe of all load-ſtones, ſince we find it produce new ones every day; and if this matter could have taken ſo many different directions in forming our ſtone in its mine, takes no fewer in the whole globe, and if changes could befal our load-ſtone like thoſe which befal the earth, by the deſtruction of ſome magnetic matter, and the formation of other where there was none before, we ſhould in courſe of time find variations on this load-ſtone, anſwerable to thoſe which befal the magnetiſm of the earth.

IX. *On the condenſation and dilatation of the air, by M.* de la Hire, *jun.* [*] *tranſlated by Mr.* Chambers.

The general rule given by M. *Mariotte*, for finding the different condenſations of the air, produced by given weights, is founded on an experiment which he relates in the firſt place, and afterwards confirms by 3 others, made with a crooked glaſs tube, one branch whereof having a foot, was ſealed hermetically; the other being as wide as one pleaſed, he put mercury into this tube, and continued the experiment, as may be ſeen in page 140, *&c.* of his treatiſe of the motion of fluids; and the experiments ariſing herefrom, gave him occaſion to eſtabliſh a general rule, and to advance, that the denſity of the air is in proportion to the weight.

My father alſo laid before the academy a general rule for the condenſation and dilatation of the air, deduced from this only ſuppoſition, that the air is heavy and elaſtic.

He made ſeveral experiments, to find in what proportion a ſpring taken in a moderate ſtate of extenſion, would be ſtretched by different weights, and found, that its extenſions were in a direct ratio of the weights; but proceeding alſo to enquire how the ſpring contracted, he found, that its condenſations were not in a direct, but a reciprocal ratio of the ſame weights, which will not be difficult to conceive, if we remember, that in ſtretching, the ſprings enlarge their bulk, in proportion as the weights are augmented; and that on the contrary, in contracting they leſſen the ſame. It was on theſe experiments, therefore, that he built his general rule, which proves perfectly conformable to that of M. *Mariotte*, and to the experiments lately made before the academy by M. *Amontons*, as will appear by the following table, where his experiments, and the reſult of the rule, are found oppoſite to each other.

---

[*] May 6, 1705.

Height of mercury in the tube.	Denſity of air by the experiment.	Denſity of air by the rule.
Inches.	*Parts.*	*Parts.*
6	$9\frac{6}{9}$	$9\frac{7}{9}+\frac{108}{135}$
12	$8\frac{2}{9}$	$8\frac{4}{9}+\frac{63}{159}$
14	$7\frac{7}{9}$	$7\frac{8}{9}+\frac{131}{167}$
18	$7\frac{1}{9}$	$7\frac{2}{9}+\frac{93}{183}$
24	$6\frac{2}{9}\frac{1}{2}$	$6\frac{2}{9}+\frac{180}{207}$
30	$5\frac{6}{9}$	$5\frac{6}{9}+\frac{207}{231}$
36	$5\frac{1}{9}\frac{1}{2}$	$5\frac{2}{9}+\frac{3}{255}$
42	$4\frac{6}{9}$	$4\frac{6}{9}+\frac{270}{279}$
48	$4\frac{2}{9}$	$4\frac{2}{9}+\frac{174}{303}$

X. *Experiments on the rarefaction of air, by M.* Amontons *, *tranſlated by Mr.* Chambers.

I filled a tube 40 inches long with mercury, and found it to contain 7 ounces, 7 drachms, 8 grains.

I filled in like manner another tube, one end whereof terminated in a large veſicle, of the figure of a cervelas, and found it hold 87 ounces, 6 drachms. The veſicle in particular, to its ſetting on to the tube, contained as much as a tube equally thick, with that of 46 inches, and 475 inches 5 lines $\frac{1}{6}$ long; the reſt of the tube, which was 29 inches long, contained as much as 36 inches 6 lines and $\frac{1}{2}$, of the ſame tube of 46 inches, ſo that the whole tube with its veſicle, repreſented a tube 511 inches, 8 lines $\frac{2}{71}$ long, and equal in thickneſs to that of 46 inches.

---

* June 10, 1705.

This tube with its veſicle, being full of mercury, I inverted it as uſual; excepting that to prevent heating the veſicle and its contents, I always handled it with a linnen cloth, which was obſerved in all the following experiments.

The lower end being immerged an inch deep in mercury, which roſe above the edge of the baſon, in proportion as the veſicle emptied itſelf, the mercury at length ſtopped in the tube 28 inches above the mercury in the baſon, which ſhewed, that the atmoſphere was then equal to ſuch 28 inches.

While the veſſel was emptying, I obſerved abundance of large air bubbles all along the tube, attempting continually to riſe, but ſtill prevented by the deſcent of the mercury, but at length they aſcended and reached the veſicle, when the mercury ceaſed falling; this air I took to be what the mercury was purging off.

To ſee whether this air did not alter the height of the mercury, I repeated the experiment with the tube of 46 inches, and the mercury ſtopped there likewiſe 28 inches above the mercury in the baſon.

Having aſſured myſelf of the weight of the atmoſphere, I filled the tube with the veſicle again, and poured a little mercury out into the tube of 46 inches, to ſee what height it would there poſſeſs. By this means I found that the air left in the tube was equivalent to 2 inches 6 lines of the tube of 46 inches, and the like of all the reſt; whether after filling the tube intirely, I meaſured the mercury poured out, or whether without filling it, I meaſured what was put in it by ſubſtracting it from the total capacity of the tube.

The natural bulk therefore being two inches, 6 lines, I inverted the tube upon which the mercury ſtopped 2 lines lower than the 28 inches; that is, 27 inches, 10 lines above the mercury in the baſon, and conſequently theſe 2 inches, 6 lines were ſpread thro' a ſpace above 190 times greater than what they poſſeſſed before, yet ſtill preſerved a ſpring equal to two lines.

Having left 18 inches, 7 lines of air, the mercury after inverting the tube remained one inch, 1 line below the 28 inches, which ſhall henceforward be the term, from whence I compute the falling of the mercury.

Having left 36 inches, 6 lines $\frac{1}{2}$ of air, the mercury ſtood 2 inches, 1 line $\frac{1}{3}$ lower.

Leaving 465 inches, 8 lines 7 $\frac{2}{1}$ of air, that is, putting only the 46 inches tube full of mercury, it ſtood 25 inches, 9 lines $\frac{1}{4}$ lower.

Putting in the tube of 46 inches twice full of mercury, it ſtood 23 inches, 9 lines lower.

Putting in thrice the ſame tube full of mercury 21 inches, 1 line lower.

Putting in four times the ſame tube full of mercury, it ſtood 18 inches, 7 lines $\frac{1}{2}$ lower.

This way of meaſuring with the tube of 46 inches appearing too tedious to be continued, I had recourſe to weighing.

Putting therefore 2 *lb.* 7 ounces, 3 drachms, 40 grains of mercury, which is 5 times the quantity of what fills the 46 inch tube, the mercury remained 16 inches, 1 line $\frac{1}{4}$ lower.

Putting in 3 *lb.* 7 ounces, 1 drachm, 56 grains of mercury, which is ſeven times the quantity, it remained 10 inches, 11 lines lower.

Putting in 3 pounds, 15 ounces, 64 grains of mercury, which is 8 times the quantity, it remained 7 inches, 11 lines lower.

Putting in 4 *lb.* 7 ounces of mercury, which is 9 times the quantity, it remained 5 inches, 7 lines $\frac{1}{2}$ lower.

Putting in 4 *lb.* 14 ounces, 7 drachms, 8 grains of mercury, which is 10 times the quantity, it remained 3 inches lower.

Putting in 5 pound, 6 ounces, 6 drachms, 16 grains of mercury, it ſunk 4 lines lower.

It muſt be obſerved, that ſuppoſing all the meaſures and weights above-mentioned exact, there muſt be ſome 5 inches, 8 lines $\frac{2}{71}$ of the veſicle tube wanting of being full, which ſhould be the natural bulk of the air in this experiment, which, however, was only found to be 5 inches, 6 lines $\frac{16}{29}$; ſo that there was an error of 1 line $\frac{1029}{2059}$ or about a line $\frac{1}{2}$, which is not very conſiderable in a length of above 511 inches, being only the $\frac{1}{6000}$ part thereof, which I only mention to ſhew, that there was no very groſs error in the meaſures and weights, and to ſignify, that we are to take the natural bulk of the air in this experiment, to be 5 inches, 6 lines $\frac{16}{29}$, in lieu of 5 inches, 8 lines $\frac{2}{71}$, which with 11 times 46 inches, make the whole length 511 inches, 8 lines $\frac{2}{71}$.

Theſe experiments made, I filled the veſicle tube intirely again, and inverting it, the mercury ſtopped as before at 28 inches.

Now to find whether theſe experiments agree with the received hppotheſis, a table may be made, wherein the product of the natural bulk, by the weight of the atmoſphere, may be expreſſed in one column, and the product of the bulk dilated by its reſpective load in the other.

My opinion was, that all the terms, which give theſe products, were only determined by the meaſures in the experiment, ſomewhat near the truth, and not to the utmoſt exactneſs, and therefore that I could not ſuppoſe any of them true, more than others, nor of conſequence infer any thing with certainty from them; and the rather as each of theſe meaſures, beſide its particular variation from the real bulk, might likewiſe vary from the hypotheſis by an error in the other 3 meaſures.

Thus, for inſtance, if the meaſure of the bulk dilated be leſs than the real magnitude of this bulk, the experiment will ſeem to deviate from the hypotheſis by the particular error of ſuch meaſure, that is, by making this dilated bulk ſmaller than the calculus; tho' it muſt be confeſſed, if there were no other error to be apprehended, this would not deſerve much notice, and the rather as it is the common practice.

But if beſides the dilated bulk being taken for leſs than it is, the natural bulk be taken for greater than it really is, this ſecond error, after inverting the tube, will add a new magnitude to the dilated bulk of the calculus, and render the difference between the calculus and the experiment ſtill more conſiderable.

And again, if the weight of the atmoſphere be taken for leſs than it is, as the ſame weight occaſions a greater change in a quantity of air much dilated, than in the ſame quantity of air when leſs dilated, the calculus will on this account give the bulk of dilated air greater than the experiment.

Laſtly, if in meaſuring the tube, its dimenſions be taken greater than they really are, this will likewiſe cauſe the bulk of the dilated air to be augmented by the calculus.

On account of theſe four errors, in reſpect of meaſures, which are no errors in the hypotheſis, I apprehended, that the bulk dilated, as found by the calculus, might differ conſiderably from that of the experiment, without any thing being deducible therefrom againſt the truth of the hypotheſis. On the contrary, this ſeemed to make it impoſſible to diſtinguiſh whence the difference beween the calculus and the experiment might ariſe, unleſs the experiment deviated very far from the hypotheſis; for in this caſe, the concluſion muſt be againſt the hypotheſis, ſince the meaſures can only deviate a little from the truth, and conſequently are incapable of producing any conſiderable difference.

I apprehended therefore that ſo long as the difference between the calculus and the experiment ſhould not be very conſiderable, it would be almoſt impoſſible to ſay, whether it proceeded from an error in the meaſures, which, by the nature of the thing, are all thrown together, or from the erroneouſneſs of the hypotheſis.

But notwithſtanding all this, ſome very knowing members of the academy, to whoſe judgment I owe all imaginable deference, having reckoned, that the quantities of the atmoſphere of the natural bulk of the air, and the length of the tube, may be taken for abſolutely true; I ſhall no longer ſtand out, but am even inclined to ſuppoſe with them, that theſe magnitudes are true.

On this footing the difference between the product of the natural bulk of the air by the atmoſphere, and the product of the bulk when dilated by its load, will be the difference which we are to ſuppoſe between the hypotheſis and the experiment; though if my ſentiment had obtained all that could have been inferred therefrom is, that as theſe products are nearly equal, we have a ſtrong inducement to believe, that the hypotheſis and the experiment do not vary from each other.

XI. *On the vernal froths, or cuckow-ſpittle, by M.* Poupart *; *tranſlated by Mr.* Chambers.

In the ſpring ſeaſon, we find a ſort of white froth, ſticking indifferently to all kinds of plants. We denominate it vernal from the time of its appearance, which is always in the ſpring, either ſooner or later, as the ſeaſon is more or leſs early.

Several naturaliſts have treated of this froth, without underſtanding its cauſe. Some will have it to be vapours, which, exhaling from certain grounds by the warmth of the ſpring, gather upon ſuch plants as they meet withal. ——— The reaſon they alledge, is, that we ſometimes find a little ſpot of ground where there is ſcarce a plant without it; tho' perhaps nothing of the kind ſhall be found for 20 or 30 miles around; which argues that all lands are not proper to produce ſuch froths.

Iſidore Hiſpalenſis takes them to be the cuckow's ſpittle; an opinion which he probably entertained from their form, and the time of their appearing and diſappearing, which correſpond pretty nearly to thoſe of the cuckow: add, that in its flight from one place to another, this bird makes a kind of rattling in its throat, as if it were going to ſpit.

Others imagine it to be the extravaſated juice of plants; and *Moufet* will have it no other than a frothy dew.

---

* June 20, 1705.

Swammerdam of all the naturaliſts has beſt entered into the reaſon of theſe froths. His doctrine is, that the graſhoppers yield them, at their mouth: and 'tis true, they are the produce of thoſe animals, tho' not at the part he mentions. So that he only ſpeaks by conjecture.

I could relate ſeveral other opinions as to the nature of theſe froths; but being all of them falſe, I ſhall dwell no longer upon them. The real caſe is as follows: In the ſummer time, we find a diminutive kind of graſhoppers, called by ſome naturaliſts *flea-graſhoppers*, from their jumping like fleas. Their hind legs do not reach above their backs, as thoſe of all other graſshoppers do, but are always folded under the belly, like thoſe of fleas. This enables them to jump very faſt, by reaſon there is no time loſt between one jump and another.

I have already obſerved, in the *Journal des Savans*, for *Auguſt* 1693, that this kind of graſshopper has a ſtiff-pointed needle, or trunk, whereby it ſucks the juices of plants. —— This obſervation ſuggeſts ſomething new, and curious; in as much as this is the only ſpecies of graſshoppers that has a trunk: all the reſt, hitherto known, have a mouth, lips, and teeth wherewith they devour herbs, and even the vine.

> *Vos locuſtæ* ——————
> *Ne meas lædatis vites: ſunt enim teneræ.*

Theſe flea-graſhoppers lay eggs; from whence, in the inſuing ſpring, ariſe young ones, which remain for ſome time incloſed in a fine membrane. This membrane is no other than a maſk, with eyes, feet, wings and other organs, which are the caſes of the like parts of the little animal incloſed therein. When it iſſues from its egg, it appears like a little whitiſh worm, no bigger than the point of a needle. Some days after it turns green, by means, as we may ſuppoſe, of the juices of the plants it is fed withal. In this ſtate it reſembles a kind of little green frog, which creeps up trees, and for that reaſon is called *rana arborea*, or the tree-frog. The inſect, tho' wrapt up in its membrane, will yet walk boldly, and at a good rate; but does not leap, or fly till it have put off its ſkin.

As ſoon as it leaves its egg, it mounts upon a plant; and clapping its *anus* thereto, depoſites a little drop of white liquor, full of air. Cloſe to this, it drops a ſecond, then a third; and continues after this rate, till it have involved or buried itſelf in a thick froth, from whence it never ſtirs, till it be delivered from its membrane, and have attained its ſtate of perfection.

To caſt forth this froth, it makes a kind of arch with half its body, the belly being the convexity thereof. This done, it begins another arch, oppoſite to the firſt, whereof the belly, which was the convex, becomes the concave part; each time it makes this double compreſſion, a little froth or bubble ſprings from its *anus*, which it afterwards enlarges, by puſhing it this way and that with its feet.

I placed ſeveral of theſe little graſhoppers upon a young mint, and found that thoſe leaves they caſt their froth upon, did not grow or advance in the leaſt, tho' thoſe oppoſite to them grew to their natural ſize. This ſhews, that the inſect lives of the juice of the plant, while incloſed in the froth.

The young graſhopper being by means hereof arrived at a certain bulk, ſtrips off its coat, or cover, which it leaves in the froth, and leaps about in the field.

The froth, we ſuppoſe, may have ſerved to defend it from the heat of the ſun, which would otherwiſe dry and wither it up; it alſo preſerves them from the ſpiders, which would ſuck them up, as I have ſometimes found.

The country people hold theſe froths a prognoſtick of fine weather. But all the matter is, that they are only found when the weather is fine; ill weather ſpoiling and deſtroying them.

XII. *Philoſophical experiments upon the refraction of muſquet-balls in water, and upon the reſiſtance of this fluid, by M.* Carre *, *tranſlated by Mr.* Chambers.

There being ſeveral perſons who doubt whether muſket-balls undergo any refraction, that is, any change in the direction of their motion when ſhot obliquely into the water, and I having aſſerted it, as a known fact, in the hiſtory of the academy for the year 1702, upon the authority of ſeveral former writers, I deſired a friend, who has been ſome time at his country-houſe, to examine into the truth of it; and the following experiments are the reſult of this requeſt.

Firſt, I ſhot a muſket-ſhot loaden with a ball once, and a ſecond time into a ſtone baſon full of water, 2 feet ½ in diameter, and 16 inches deep; the firſt time under an angle of 20 degrees, and the ſecond under an angle of 80; but I could not perceive whether the balls underwent any change of direction, by reaſon of the great effort of the water againſt the ſides of the baſon, which always diſplaced the piece of paſt-board, which had lodged there for a mark. This effort is ſo violent, that upon ſhooting three times into tubs of water, they were all inſtantly burſt; and in particular it was always the loweſt hoops that the water made give way. To ſatisfy myſelf that it was really the great motion and effort of the water that burſt the veſſels, and not the ball by paſſing thro', I provided a ſquare cheſt about a foot high, and ½ a foot thick, the 4 boards whereof, which formed the length, were each an inch thick, and the two at the end, each 2 inches thick, that the others might be well faſtened thereto, with ſtore of nails, then filling it with water by a little hole, I exploded my piece, upon which the ball perforated the boards very exactly without burſting them; but the water was ſhook ſo violently, that it tore the boards from each other, and burſt the cheſt.

---

* July 11, 1705.

Secondly, To make a more preciſe experiment on the refraction of a ball in water, I filled a ſtone baſon therewith, whoſe length on the inſide was 3 feet, 3 inches, its breadth 1 foot, 8 inches, and depth 1 foot, 1 inch, here repreſented by ABCD *. To the ſide BD, I faſtened a board to receive the balls, another board EF preciſely in the middle, and another CD upon the bottom to cover it intirely; the boards were all ſtrongly faſtened, that the ſtrokes of the ball might not ſhake them. Over the ſide CA, I fitted a piece of paſtboard GA perpendicular to the horizon, and the muſquet HN was fixed 8 feet from the baſon, upon exploding it the ball pierced the paſtboard in K, and was found flatted like a ſhilling at M. I ſhot a ſecond time,

* Plate V. Fig. 5.

and found the ball broke into 3 pieces, which were likewiſe flatted, without appearing to have ſtruck againſt the board EF. I ſhot two balls with a ſtronger charge of powder; but they were neither of them to be found, either at the bottom of the baſon, or in the boards. These balls were a 3d of an inch in diameter, and being made on purpoſe for the piece, filled it very exactly.

Thirdly, To try anew the experiments of refraction, I bethought myſelf of a way to get an inſight into this flattening of the balls; to this end, I procured a reſervoir 10 foot ſquare, and fitted therein two boards parallel to the horizon, and to each other, and about a foot apart, the upper one making the ſame plane with the ſurface of the water, then ſhooting two balls upon this board under an angle of 30 degrees, and with an equal charge of powder; the firſt, with the piece uſed before, whoſe barrel was 3 feet, 2 inches ½ long, and the bullet 3 lines ¼ diameter; the ſecond, with another piece, whoſe barrel was 3 feet, 10 inches, 3 lines, and the ball 7 lines in diameter. Here the large ball pierced the two boards, and conſequently traverſed the whole extent of water between them; whereas the ſmall one only pierced the upper board, and was found quite flatted on the lower.

Fourthly, But to ſatisfy myſelf whether there be really a refraction, I made uſe of the ſtone-baſon, deſcribed above, and prepared after the ſame manner; then faſtening my piece upon two reſts, one of them 5, and the other 7 feet diſtant from the baſon, I nailed it down, ſo as to make it remain in the ſame ſituation after the exploſion, as before. It now made an angle of 20 degrees with the horizon, or the ſurface of the water in the baſon, and was charged with 3 penny weights, 20 grains of powder, and a ball 7 lines in diameter, weighing 17 penny weights, 6 grains, ſhooting it off, the ball, after piercing the paſt-board in K, and the board EF in P ſtuck in the point R, where I found it faſt. Upon this, emptying the baſon, I drew a thread from the middle of the ball in R, thro' the holes P and K, to the middle of the mouth of the piece, and found that the thread paſſed exactly enough thro' the centres of all the holes.

I repeated the ſame experiment, by ſetting the piece a little on one ſide, that the ball might paſs thro' a freſh place. The effect was, that piercing the paſt-board ½ an inch from K, and the board EF ½ an inch from P, it fixed likewiſe ½ an inch from R; ſo that the centres of the two balls wanted not a line of being in the ſame parallelliſm to the horizon; hence it may be inferred, that if there be a refraction, it is not ſenſible.

Being willing to try whether the ball of 7 lines diameter would be flattened by increaſing the charge of the piece, I uſed 7 penny weights 6 grains of powder, and found accordingly the ball about M flattened on one ſide; it had ſtruck the board EF ſlightly; but this could not have occaſioned its flattening, ſince a ball will pierce 3 or 4 boards without loſing much of its ſphericity. Putting the ſame charge of powder in the former piece, I found the ball about M, cloven into two parts, each of them flatted unequally, tho' without having ever touched the board EF. Shooting a ſecond time with half the charge of powder, the ball did not reach the board EF, nor loſt any thing conſiderable of its ſphericity.

Fifthly, To procure further ſatisfaction as to the flattening of the balls, I ſpread a linnen cloth parallelly to the horizon, under two feet depth of water, in a baſon 40 feet in diameter, and 6 feet deep, then charging my piece very high with powder, and loading it with a ball of 7 lines diameter, I diſcharged it againſt the linnen, and found the ball flatted thereon, tho' very unequally, and of an irregular figure; then charging the piece anew with $\frac{1}{3}$ more powder, the ball divided itſelf into ſeveral little pieces, whereof I found five upon the linnen, moſt of them about the bigneſs of a lentile, but differently ſhaped.

I then ſhot another ball perpendicularly to the ſurface of the water, which was found flattened very regularly, being almoſt as round and flat as a ſhilling; whereas all thoſe which had been ſhot obliquely had flattened irregularly.

It may be proper to obſerve, that in ſhooting a ball into the water, it riſes or rears itſelf above the ſame more or leſs, as the charge of powder is greater or leſs, and I have even ſeen it riſe 20 feet high.

It will not be very difficult to aſſign the cauſe of theſe ſeveral effects, with regard to which I muſt obſerve, that 4 penny weight of powder drive the large ball of 7 lines a conſiderable ſpace in the water, without taking any thing from its ſphericity; that with 8 penny weight of powder, it loſes $\frac{1}{2}$ its ſphericity; and with 12, the whole; and laſtly, with 16, breaks into ſeveral parts.

Reflections upon the experiments above.

There are two things to be conſidered in theſe experiments, which I can warrant to have been made with great care and exactneſs: the firſt, that the ball ſhot in an angle of 20 degrees changed nothing, at leaſt as to ſenſe of the direction of their motion. The ſecond, that theſe balls when exploded with a great charge of powder, turned flat as they entered the water.

Firſt, It ſhould ſeem at 1ſt ſight agreeably to the common notion of the reſiſtance of fluids, that a ball ſhot into the water under an angle of 20 degrees, or even a greater angle, ſhould not only change its direction but even rebound; for as the quantity of matter in all bodies is proportionable to their weight, and the reſiſtances of fluids are in the ſame *ratio* as their denſities, and the denſity or weight of water is to that of air, as 800 or 850 to 1, according to ſome experiments, or as 1175 to 1, according to thoſe of the academy *Del Cimento*; the water muſt make 800 times more reſiſtance than air, and conſequently diminiſh the velocity of the ball by the ſame quantity. Now 'tis eaſy to ſhew purſuantly to the reaſoning of M. *Deſcartes*, and ſeveral others, that if the ball, upon meeting the water looſe half its velocity, it muſt rebound.—— For ſuppoſe a circle * EGD, whoſe diameter EF repreſents the ſeparation of the ſurfaces of air and water, and ſuppoſe the angle ACE to be 20 degrees, whoſe ſine will be AB, and CB the ſine of its complement, the ball now being driven from A to C, we may conſider its motion as compoſed of the vertical one AB, and the horizontal one BC, as CB therefore is much greater than half of CE. If the ball loſt half its velocity upon meeting the water, it ſhould of conſequence rebound; but experience ſhews us, that it does not.

It muſt therefore be made appear, that the ball loſes ſo little of its vertical motion, that it not only ſhould not rebound, but ſhould not even loſe any thing ſenſible of its former direction.———

---

* Fig. 6.

In order hereto, we are to obſerve, that 'tis when the ball enters the water that its direction is to be changed, and that its motion in AC, being compoſed of a horizontal motion, equal to BC, and a perpendicular one, equal to AB, we are only to conſider this latter, ſince the former makes no oppoſition thereto; and again, that this ball finds no more reſiſtance on one ſide than on the other, till ſuch time as half its magnitude is immerged under water; for which reaſon, if the bulk of water, whoſe place it poſſeſſes, were equal in weight to half the ball, as would be the caſe, if the ball were ſhot into melted lead, 'tis evident the ball would loſe near half its velocity; but it being only water that oppoſes its motion, and the weight of lead being to that of water, as 12 to 1, if in meeting with the bulk of water equal to its own, it muſt only loſe $\frac{1}{12}$ part of its motion, ſince, as already obſerved, the reſiſtances of fluid bodies to their being ſeparated are in the *ratio* of their denſities, or weights: but as here we have only half to conſider, the ball muſt only loſe $\frac{1}{24}$ part of its vertical motion; that is, a 24th part of AB, which being but a ſmall matter, the change of direction muſt neceſſarily be inſenſible. If it be alledged, that this 24th part ſhould be conſiderable enough to be perceived, by reaſon it muſt be referred to this, that the ball has two boards and a paſt-board to pierce, which may occaſion ſome little alteration in its motion, perhaps the motion of gravity of the ball might likewiſe be conſidered, which has a tendency to carry it directly downwards, and conſequently to prevent its change of determination from being ſenſible; but the great velocity of the ball, and the little ſpace it paſſes, muſt render this next to nothing. 'Tis otherwiſe however

ever with the rays of light, by reaſon of their paſſage from air to water, or other tranſparent mediums. There is no proper local motion, but only certain inequalities of preſſion, or reſiſtance, in the different mediums, as has been explained elſewhere; ſo that this does not hinder us from concluding, that the rays of light always paſs more eaſily into the denſeſt mediums, and that 'tis on this account they approach towards the perpendicular.

It muſt be obſerved however, that if a ball be ſhot very obliquely, ſo that the angle of incidence only contains a few degrees, its horizontal motion being now very great with regard to the vertical one, it may meet ſuch a multitude of particles of water at the ſame time, as may prevent its entering, and oblige it to rebound; and ſuch is the caſe, when we make what the boys call ducks and drakes.

The ſecond thing to be conſidered in theſe experiments, is the flattening of the balls upon meeting the water; and at firſt ſight, it may ſeem ſurpriſing, that a fluid body, ſuch as water, which yields and divides ſo readily, ſhould make a reſiſtance like a ſolid body; but if the great velocity of the ball be conſidered, 'twill appear poſſible for it to meet ſuch a quantity of particles of water at the ſame time, as that their reſiſtance ſhall be equivalent to that of a hard body, and turn it flat. In drawing the hand with ſome velocity thro' the water, we find a certain reſiſtance; and if it be drawn with twice the velocity, the reſiſtance will be fourfold, ſince we have twice the number of particles to move in the ſame time with a double velocity; and if the ſame hand be drawn with thrice the ſwiftneſs, the reſiſtance will be 9 times greater; ſo that the reſiſtances increaſe in the *ratio* of the ſquares of the velocities. Hence the velocity of the hand may be ſo great, as that the partieles of water it meets in any given time may afford it a reſiſtance equal to that of a hard body; in effect, if the hand be ſtretched parallelly to the ſurface of the water, and ſtruck with nimbleneſs againſt this ſurface, the reſiſtance will be ſo great as to be painful, and I remember, that beating the water ſtrongly one day with a ſtick, it broke in my hand.

From hence 'tis eaſy to account for the flattening of muſquet-balls ſhot into water, and what confirms the theory is, that the higher the piece is charged, the more flattened are the balls found; for that having a greater velocity, they meet more particles of water in the ſame time.——— And hence we may account why an end of a candle being loaded in a fowling-piece will pierce a thick plank of wood.

XIII. *A compariſon between our obſervations on the barometer, and thoſe made by F.* Sebaſtien Truchet, *by M.* Maraldi *.

Among the obſervations on the barometer, which F. *Sebaſtien* laſt related to the academy, we have chiefly conſidered thoſe which he made at *Clermont*, and on the ſummit of *Mount d'Or*, the higheſt mountain in *Auvergne*; the perpendicular height of which, we determined above the ſurface of the ſea by the angles, of the meridian, but could not make the barometrical experiment upon it, it being at that time covered with ſnow.

---

* July 5, 1705.

This year 1705, *June* 8, at 4 in the afternoon, F. *Sebaſtien* obſerved on the ſummit of *Mount d'Or*, that the quickſilver in the barometer was ſuſpended at 22 inches, 2 lines.

The ſame day at noon, we found the height of quickſilver at the obſervatory 27 inches, 9 lines ½, and at $7^h\ 24'$ in the evening, it was 27 inches, 9 lines ¾, having encreaſed but ¼ of a line ſince noon.

Between the 22 inches, 2 lines height of quickſilver obſerved at *Mount d'Or*, and the 27 inches, 9 lines ¾, obſerved at *Paris*, the difference is 5 inches, 7 lines ¾, which the quickſilver at the obſervatory kept higher than at the top of *Mount d'Or*. We have found by our experiments, that the quickſilver on the ſea ſhore is commonly 4 lines ⅙ higher than at the obſervatory. The quickſilver was therefore 5 inches, 11 lines ⅔ lower on the mountain, than it would have been at the ſame time on the ſea-ſhore.

In the memoirs of the academy of 1703, we ſaid the perpendicular height of *Mount a'Or* above the ſurface of the ſea was 1030 toiſes: but M. *Caſſini* the ſon, by a more exact calculation, found it afterwards to be 14 toiſes higher; ſo that its perpendicular height above the ſurface of the ſea will be 1047 toiſes, to which we have found as above, that a variation anſwers of 5 inches, 11 lines ⅔ in the height of the quickſilver.

By the progreſſion founded on the experiments related in the memoirs of the academy for the year 1703, the quickſilver ſhould, to this height of *Mount d'Or*, ſink 5 inches, 7 lines, which is 4 lines leſs than by the obſervation.

M. *de la Hire* has communicated to us the obſervations which he made the ſame days on the barometer: and tho' his barometer and ours are on the ſame floor of the obſervatory, yet theſe obſervations ſometimes differ a little; whether it proceeds from their having been made at different honrs of the day, or from any other cauſe.

By M. *de la Hire*'s obſervation *June* 8, at $\frac{1}{2}$ after 5 in the morning, the height of the barometer was 27 inches, 8 lines $\frac{1}{3}$. If this obſervation is compared with F. *Sebaſtien*'s, in the ſame manner as we did ours, there will be found between the level of the ſea and *Mount d'Or*, a variation of 5 inches, 10 lines $\frac{1}{2}$ in the height of the barometer, which agrees within 3 lines with what is given by the progreſſion.

F. *Sebaſtien* made another obſervation with the barometer at *Clermont*, near the convent of the *Minims*, which is that part of the city where M. *Perier*, in the year 1641, made his experiment on the barometer, the ſame day whereon he carried it on *Puy de Domme* to find the variation of the barometer, which anſwers to theſe two heights.

June 10th, at 6 in the evening, F. *Sebaſtien* found the height of the quickſilver at *Clermont*, to be 26 inches, 6 lines. At *Paris*, by obſervations made before and after, it was 27 inches, 10 lines high.

Therefore the difference of the quickſilver between *Clermont* and *Paris* was 1 inch, 4 lines.

We have ſaid above, that between *Paris* and the level of the ſea, there is in the height of the quickſilver a variation of 4 inches $\frac{1}{6}$.

Between *Clermont* therefore and the ſurface of the ſea, there muſt then have been 1 inch, 8 lines $\frac{1}{6}$.

By M. *Perier*'s obſervations made in 1641 between the convent of the *Minims*, and the top of

Puy deDomme, there was a variation of 3 inches, 1 line $\frac{1}{2}$ in the height of the quickſilver.

From the level of the ſea to the ſummit of *Puy de Domme*, there muſt, according to theſe different obſervations, be in the height of the quickſilver a variation of 4 inches, 9 lines $\frac{2}{3}$, which anſwers to 810 toiſes perpendicular height, from the ſummit of *Puy de Domme* to the level of the ſea. By the progreſſion ſettled in the memoirs, 55 lines $\frac{1}{4}$ variation of quickſilver agrees with this height, which is but two lines different from the obſervation.

But by M. *de la Hire*'s obſervation made on the ſame day *June* 10, the barometer was 27 inches, 8 lines high; and making the ſame reductions and compariſons, as in the preceding obſervation, there will be between the ſea ſhore, and the ſummit of *Puy de Domme*, a variation of 55 $\frac{1}{3}$ in the height of the quickſilver, as taken from the progreſſion ſettled in the memoirs.

XIV. *Remarks on ſome experiments made with ſeveral barometers, and on the light made by one of them, which was agitated vertically, by M.* de la Hire *the ſon* *.

We have two barometers in the obſervatory; one of which has a tube of 1 line $\frac{1}{2}$ inſide diameter, and is 36 inches $\frac{1}{2}$ in height; and conſequently when the quickſilver is at 28 inches, there remains 8$\frac{1}{2}$ void. It is this which we commonly make uſe of. The other is 2 lines inſide diameter, and 32 inches $\frac{1}{4}$ in height, and conſequently there is but 4 inches $\frac{1}{4}$ remaining above the 28 inches. It was this barometer M. *Picard* made uſe of, and which was the firſt wherein a light was obſerved, by agitating it vertically. It ſtill continues to give as uſual a very great one: the other gave none, though filled with the ſame quickſilver; this is moſt certain, for it was very often looked upon. Yet within theſe few days having been agitated, it was ſeen to give almoſt as much light as the other.

---

* Aug. 12, 1705.

This barometer of M. *Picard's* was emptied and filled many times without any precaution to cleanſe the quickſilver and the tube; yet it always gave the ſame light as at firſt. But we obſerved, that though this light was very lively, ſo as to be ſeen at candle-light and moon-light, by expoſing the tube to it: yet if in the day-time a room was ſhut exactly cloſe, ſo that it was quite dark, and the barometer a little time after was therein agitated, no light thereof was to be ſeen, which made us at firſt imagine, that it gave no light in the day-time: but endeavouring to be more certain of this experiment, we continued in the room, where it gave no light for above a quarter of an hour; and then agitating the barometer, which during that time we had put in the ſun, we ſaw its light as great as in the night. This laſt experiment deſtroyed the imagination we had conceived, and gave us to underſtand, that the retina requires a conſiderable time to loſe that vibration, which is cauſed in it by the light of the ſun.

The height of the quickſilver in theſe two barometers differs always 3 lines ½, which M. *Picard's* is higher. We have lately made another. The quickſilver was ſtrained through a fine clean linnen, and the tube, which was of 3 lines diameter, and 35 inches long, was well cleanſed with ſpirit of wine, and afterwards throughly wiped with very dry linnen drawn through it; and after having very carefully filled it, that no viſible bubble of air might be left therein, we obſerved, that the quickſilver kept at 1 line $\frac{2}{3}$ lower than in M. *Picard*'s barometer, and very near the ſame quantity higher than in the other.

But in trying this barometer, we obſerved that after having filled the tube with quickſilver, and evacuated all the air, and dipped the open end of it when ſtopped with a finger into the quickſilver, the tube being at firſt very much inclined; when it was raiſed, and the void began to appear above, ſome almoſt imperceptible little bubbles of air were diſcerned, which ſuddenly became as big as little peaſe, and entered into the *vacuum*; ſome of them being confined between the quickſilver and the tube, and others ſeeming to come out of the quickſilver, and performing the ſame effect as if it was boiling. We alſo obſerved, that theſe bubbles which came out of the quickſilver by raiſing the tube, when they became a little big, diſappeared by lowering of it, and ſeemed to re-enter into the body of the quickſilver; for on that part where they diſappeared, nothing was to be ſeen againſt the tube. This barometer when agitated gave no light.

There is no queſtion but that all theſe bubbles, as well thoſe confined between the quickſilver and the tube, as thoſe which ſeemed to come out of the quickſilver, were little particles of air encloſed and confined therein, which being then diſburthened from all the weight of the atmoſphere, and from the height of the quickſilver, which compreſſed them in the tube when the open end was above, occupy a very great bulk, with reſpect to that which they before occupied;

cupied; and it is certain, that the more ſlender and the longer the tube is above 28 inches, the more of theſe bubbles will eſcape into that ſpace which the quickſilver quits; all the quickſilver which occupied that place having there purged itſelf from the air. Therefore it will appear, that tubes muſt be taken of a length proportioned to the places where the barometers are to be put, and they are to be left but one inch above the greateſt height of the quickſilver in the place they are in, and that they be about 3 lines in diameter, rather more than leſs, and the quickſilver well purged from the air. With theſe precautions, I think, barometers may be made as exact as poſſible.

We now make no longer doubt but that the barometer, which we commonly make uſe of, whoſe tube is ſlender and too long above the 28 inches, has many of theſe particles of air confined in the quickſilver, and between the quickſilver and the glaſs, which being diſengaged when it is tried, have occupied a conſiderable place in the top of the tube, and that this is the real cauſe why the quickſilver therein is lower than in thoſe which are larger and ſhorter, wherein the bubbles, when dilated, have not ſo ſenſible an effect: from the reaſons above-mentioned,

It muſt be obſerved, that thoſe circumſtances which were thought neceſſary to make barometers luminous, ſeem, by what we have now ſaid, to be deſtroyed.

XV. *On the height of the quickſilver in the barometers, by M.* Amontons †.

This experiment is of great conſequence; for it lays us under the neceſſity of examining over again all the obſervations made on the barometer to this day.

It has hitherto been thought, that the height of the quickſilver in barometers was always viſibly the ſame in the ſame place, and it was far from being imagined, that, with glaſſes very near alike filled with the ſame care, and the ſame quickſilver, the heights of this quickſilver ſhould differ from one another in the ſame place, and at the ſame time, 18 lines, or thereabouts. Yet this is what I ſhall make appear to the company, after having obſerved that one principal reaſon, which may have hindered this *phænomenon* from being perceived, is, that moſt of thoſe who conſtruct barometers, have unfortunately neglected to place thereon ſuch graduations, as truly expreſs the heights of the quickſilver, and have almoſt always in the room of theſe ſubſtituted arbitrary graduations, which bear no relation to the heights of the quickſilver; occaſioned, without doubt, from a true ſenſe of the difficulty there is in making theſe ſorts of inſtruments uniform, and that it would enhance the price, and leſſen the ſale of them. Thus intereſt is frequently an obſtacle to the diſcovery of truth.

It appears, therefore, that it was not without great reaſon, that in my barometers I rejected theſe kind of arbitrary graduations, being fully perſuaded, that it is impoſſible to make a profitable uſe of barometers by exact obſervations, if the degrees are not divided into parts, which expreſs the inches and lines of the height of the quickſilver with which they are filled; and alſo, if they are not regulated by the ſame barometer, which ſhould be as a ſtandard and rule for them, without which all muſt be uncertain, and lead to error.

---

† Aug. 14, 1705.

Seeking the reaſon of the *phænomenon* which I relate, it is difficult to avoid attributing it to the inequality of the pores of different glaſſes, which give more or leſs paſſage to the particles of air, according as they are more or leſs open; which ſeemed the more likely to me, being certain, that the glaſſes of the two tubes, with which I made this experiment, are different in quality.

We owe this diſcovery to the lord chancellor. He has a ſimple barometer mounted in the *Engliſh* manner, that is, one of thoſe which has two little copper-plates, on which are marked the different diſpoſitions which may happen in the air, as fair, changeable, rain, *&c.*

My lord chancellor had, for a conſiderable time, experienced with ſatisfaction the indications of his barometer; but at laſt it being out of order, he applied to M. *Homberg*, who mended it for him. Since when the variations of this barometer have always been in the lower parts of the copper-plates, that is, in thoſe parts which indicated nothing but rain, wind, and ſtorms. My lord chancellor obſerving nothing like this in the diſpoſition of the air, ſent for me to examine his barometer. The firſt thing I did, was to ſee by inclining it, whether the *vacuum* was well made; and having found it as well as poſſible, and that the quickſilver had all the freedom of motion which could be deſired, I anſwered my lord, that I ſaw nothing could hinder its having effect. He then explained to me what he had obſerved in the manner I have mentioned; and I begged leave of him, that I might carry his barometer home with me to examine it at more leiſure, which he granted. I meaſured, as ſoon as poſſible, the height of the quickſilver, and finding it only 26 inches, 6 lines, while 3 other glaſſes which were in uſe, and in which the *vacuum* was not even ſo perfect as in that, were at 28 inches, I at firſt thought this might proceed from the quickſilver, which had perhaps a more than ordinary weight; which made me immediately take the barometer to pieces, and having filled one of my tubes with this very quickſilver, it ſtopped at 28 inches, as in the 3 others which were in uſe. I then filled the glaſs of the barometer with other quickſilver, but it always ſtopped at 26 inches, 6 lines; which gave me then no room to doubt, and I knew that this effect could only proceed from the glaſs. I then changed this glaſs, and put another into the barometer: and in this new glaſs the quickſilver kept up 18 lines higher than in that which I had taken away; ſo that as the barometer played before in the lower part of the copper-plates, it would now have played in the upper parts, had I not raiſed the plates about 4 or 5 lines; and my lord ſtill thinks, they ought to be raiſed higher: whence it may be ſuppoſed, that the glaſs which I took away, was not that which was at firſt there, in which the *vacuum* was probably made of a mean height of what is obſerved in theſe.

XVI. *A continuation of the obſervations on the height of the quickſilver in the barometers, by M.* Amontons *.

Inſpecting the glaſs of my lord chancellor's barometer, having gueſſed that it was bought of ſieur *Deville* enameller, I went to find him when I left the academy; having aſked him, he ſaid it was ſo. I cauſed him to make me four others; that is, two of the ſame glaſs, and two of another ſort of glaſs; when I had filled all of them with quickſilver, and alſo thoſe two which I had made uſe of in the experiment at the academy, the quickſilver ſtopped at different heights in them all.

The greateſt height was 28 inches.

The ſecond, half a line leſs. This was that glaſs of the academy wherein the quickſilver remained higheſt.

The third, one line $\frac{1}{2}$ leſs.

The fourth, ſeven lines leſs.

The fifth, ſeven lines $\frac{1}{2}$ leſs.

The ſixth, ten lines leſs. This was the glaſs wherein the quickſilver at the academy remained loweſt.

So that the difference in the ſecond height, which, in the morning, I had found to be 18 lines, and in the afternoon, at the academy more than 19 lines, was at $\frac{1}{2}$ an hour after 8 in the evening but 9 lines.

I left all theſe glaſſes in trial, and the next day found them of the ſame heights. But this great difference of 18 lines, which I now found to be only 9 lines puzzled me. As nothing to my knowledge had happened to the quickſilver, except that it had been much handled, I judged, that perhaps the dirt and moiſture of the hands might partly have ſtopped the pores of this glaſs. I therefore emptied out the quickſilver to waſh the outſide, and ſcour it as well as I could with ſpirit of wine: but after having done this, and filled the glaſs again with its own quickſilver, I found this difference ſtill diminiſhed 1 line ½, which made me reſolve to meddle no more with it. I have left it ever ſince, and it has only varied like all others, that is, it has fallen about 2 or 3 lines.

---

* Aug. 19, 1705.

As all this is very unaccountable; in order to endeavour to give ſome light into a thing wherein there is ſo little, with deference to the opinion of the company, mine would be to chooſe among a number of glaſſes, thoſe which being filled with quickſilver give heights of ſenſible difference from one another, and apply them all to the ſame graduation; or, which is the ſame thing, on the ſame vertical plane, at the bottom of which ſhould be a kind of common trough, full of quickſilver, in which they muſt all dip. Above this trough, beginning at the ſurface of the quickſilver, ſhould be 29 or 30 parallel lines drawn from inch to inch: the 4 or 5 laſt ſhould be ſubdivided from line to line by other parallels.

It would be alſo convenient to add to all theſe glaſſes another of equal length, but uniform from one end to the other, ſealed hermetically at each end, in which ſhould be about 28 inches of quickſilver; and in the empty ſpace thick air.

This tube would ſerve to ſhow the effect of heat on the quickſilver, and whenever the quickſilver in the other glaſſes has no other motion but what is equal to this, there ſhould be no regard had to them, as not being an effect of the weight of the atmoſphere. Such a tube, by rights, ſhould for the future accompany all ſimple barometers.

This whole machine conſtructed in this manner, ought to be carefully obſerved for a conſiderable time, and thereby a certainty might be gained.

1ſt, Whether the variations happen in all the glaſſes at the ſame time.

2dly, Whether they are equal in all of them, or whether they are not rather proportionable to the heights of the quickſilver, with which each glaſs is filled; which ſeems very likely, if it is true, that the pores of the glaſs give way to the parts of air, which are ſmall enough for that.

XVII. *A continuation of the obſervations on the height of the quickſilver in the barometers, by M.* Amontons †.

M. *Homberg* having informed us, that he had waſhed the tube of the lord chancellor's barometer with ſpirit of wine, it occaſioned ſome to ſuſpect this might be the cauſe why the quickſilver in this tube remained lower than in the others; which ſeemed to me ſo much the more probable, as I recollected, that when I firſt examined this barometer, the little reflection of light, which the bending at the top of the quickſilver commonly makes, appeared to me more obſcure than uſual; this being occaſioned, as I now imagine, from a little of the ſpirit of wine remaining in the tube.

---

† Aug. 22, 1705.

That which hindered me from perceiving it, was,

1ſt, That the quickſilver ſeemed to me very clear through the whole length of the glaſs, without any little bubbles of air ſuch as are uſually formed when the tube is not very dry.

2dly, Becauſe having, as I have already ſaid, held the barometer ſloping, I found the *vacuum* as well made, as in thoſe glaſſes which are filled in the beſt manner.

And alſo this great difference which I at firſt found in the heights of the quickſilver of this barometer, from that of my other glaſſes, and which diminiſhed by degrees, as I emptied and filled again this tube, ſeemed to me a confirmation of the fact, it being impoſſible this effect ſhould be any other than a conſequence of the diſſipation of that little ſpirit of wine.

In ſhort, to be convinced and ſatisfy what had been reſolved, I waſhed the inſide of this tube with ſpirit of wine, rubbing it pretty hard with a little cotton faſtened to the end of a braſs wire; then letting it drain a whole night (which ſeemed to me long enough, conſidering how eaſily ſpirit of wine is known to evaporate) I filled it with quickſilver, together with the other tube, in which the quickſilver had always kept very high, which I did not clean, though it appeared very foul. After which I really found there was the 19 lines height of quickſilver difference between theſe two glaſſes, which I had found at the academy, and the little edge of the quickſilver obſcured.

Though hereby the fact ſeems to be ſufficiently cleared; yet the difficulty of explaining the cauſe thereof ſubſiſts. For in ſhort, it no ways appears, that this ſpirit of wine ſhould be reduced into air, as might be imagined ; for this air muſt have had the ſtrength of a ſpring, equal to 19 lines of quickſilver, and the glaſs being put into a horizontal ſituation, this air would alſo have occupied therein near 5 lines, whereas nothing of this is even now to be ſeen ; yet the tube makes an angle of about 45 degrees with the horizon, or thereabouts.

Beſides the new tubes, wherein the quickſilver kept 6 or 7 lines lower in ſome than in others, and whoſe difference diminiſhes alſo by degrees as they are emptied and filled again with the quickſilver, without any reaſon to ſuſpect that they ever had any ſpirit of wine in them, gives room to imagine that the ſpirit of wine occaſions a leſs height in the quickſilver, only by making the glaſs cleaner, and preventing the quickſilver from fouling the inſide of the tube, which perhaps ſtops part of the pores of the glaſs. But why in barometers, which have been long mounted, this filth does not continue to ſtop intirely theſe pores, is what cannot eaſily be accounted for ?

It is true, that this obſtruction of the pores of the glaſs, ſeems to be made only in proportion, as theſe glaſſes are emptied and filled again with quickſilver ; and perhaps one and the ſame glaſs has not often enough been emptied and filled again for this to be perceived.

However it is, it ſtill appears difficult to explain the phænomenon in queſtion, except by ſuppoſing that there paſſes a greater quantity of the leaſt particles of air through thoſe glaſſes, whoſe pores are moſt open and leſs incumbered, as I have already ſaid in my firſt obſervations, and that perhaps more conſiderable differences would be found, if tubes were uſed which were made of a matter different from glaſs.

XVIII. *Experiments on the capillary tubes, by M.* Carré [*]; *tranſlated by Mr.* Chambers.

Moſt modern authors, who ſpeak of the weight and ſpring of the air, take notice of the experiments made with capillary tubes, and endeavour to explain why water riſes therein far above its level; and the farther as the diameter of the capillary tube is ſmaller, the opinions are very much divided hereon: Some will have this aſcent owing to the preſſure of the atmoſphere, being greater on the ſurrounding water, than within the tube; others account for it from the air in the tube, not being at liberty to move and act with all the force of its ſpring againſt the water riſing therein; others hold, that it is occaſioned by the water's moiſtening the inſide of the tube adhering thereto, and being in ſome meaſure ſuſtained thereby; ſo that the lateral columns of the water, which ſurrounds the tube, having more force, that is, relative weight, than the columns of water in the tube oblige them to aſcend.——— But in phyſical matters, it being experiment alone that is to decide. I have been induced, on account of the great number of celebrated authors, which have ſpoken hereof, to make ſeveral experiments on this head, and the generality of them in conſort with M. *Geoffroy.*

1ſt, We took 3 capillary tubes, the largeſt whereof was $\frac{1}{3}$ of a line in diameter; the 2d, $\frac{1}{6}$ of a line, and the ſmalleſt, $\frac{1}{10}$. Theſe we plunged in water, and to wet them the better, paſſed ſome of it quite through them; then putting them in a vertical ſituation, the water roſe above its level in the 1ſt; 10 lines in the 2d; an inch $\frac{1}{2}$, and 2 inches $\frac{1}{2}$ in the ſmalleſt.

---

[*] Aug. 22, 1705.

Then taking the 3 tubes, we ſtopped one of their ends with a piece of wax, and faſtening them one after another to the ſcale of a very nice balance, letting the open end dip in a veſſel of water underneath, and having diſpoſed them thus, we put them in perfect equilibrium. The piece of wax at the upper orifice, was intended to prevent the water from entering them that way, and was removed when the tubes were balanced and laid in the ſcale with the tube, to prevent any alteration in the equilibrium; immediately whereupon the water roſe in the tubes, to the heights above ſpecified. Now I argued before the experiments, that if the water roſe in the tubes by an unequal preſſure of the air, the equilibrium would remain the ſame after as before; but if the aſcent were owing to the water's moiſtening and ſticking to the ſides of the tubes, the water thus adhering, muſt be conſidered as a little weight ſuperadded to the tube, and conſequently the equilibrium would be deſtroyed.

The event was, that the water, as it roſe in the little tube, made no alteration in the equilibrium, but that the equilibrium was broke, as it roſe up the middle tube, and ſtill more ſo in the large tube where the ſcale with the tube inviſibly deſcended. —— Hence it ſhould ſeem, after the reaſoning above-mentioned, that the cauſe why the water aſcended in the tubes, was its ſticking to their inſides, and that the queſtion was decided; but reflecting, that when one of the ends is cloſed with wax, the tube and the air within it, may be conceived as one ſingle body, whoſe bulk is lighter than that of the water, whoſe place it poſſeſſes, and conſequently muſt remain in the ſame equilibrium ; but that the tube being afterwards unſtopped, and the air thus ſet at liberty to come forth, and the water to enter, we are henceforth only to conſider the proper matter of the tube, whoſe bulk is heavier than an equal bulk of water ; ſo that this cauſe alone muſt deſtroy the equilibrium, and therefore nothing can be determined from theſe experiments, as to the real cauſe why water aſcends in capillary tubes.

Taking the largeſt tube, and immerging it firſt in ſpirit of wine, the liquor roſe therein about 3 lines ½ above its level, and plunging it a ſecond time, it roſe 4 lines.———Then immerging the ſame tube in common water, it roſe 5 lines ¾ ; and immerging it a ſecond time, 7 lines ¼ ; the third time, it roſe 10 lines. —— Immerging it next in oil of turpentine, the liquor roſe 4 lines above its level ; then immerging the ſame tube thus moiſtened with oil of turpentine, after firſt paſſing ſpirit of wine through it to cleanſe it in ſpirit of wine, that liquor did not riſe ſo high as the level of what was in the veſſel, the reaſon whereof was apparently owing to a little drop of the liquor adhering to the inſide of the tube. — The tube hereupon was immerged in oil of tartar per deliquium, which roſe upwards of 5 lines above the level ; and being immerged a ſecond time, it roſe 6 lines. —— It was immerged in ſpirit of nitre, which roſe 4 lines. —— Then in oil of olives, which roſe 5 lines, the tubes wherewithal theſe experiments were made being 12 inches ½ long.

Another was then taken of the ſame diameter 9 inches ½ long, which being immerged in common water, the fluid roſe, as in the former, about 10 lines above its level ; immerging it then in ſpirit of wine, it roſe 4 lines. — Whence it appears, that the different length of the tube makes no difference in the aſcent of the fluid.

Immerging this tube in mercury, it reſted below the level, and immerging one of the ſmalleſt ſize, the mercury had no viſible riſe.

Immerging another tube, 15 inches long $\frac{1}{6}$ of a line in diameter, the ſpirit of wine roſe therein about 12 lines. —— Immerging it in common water, it roſe 2 inches, 5 lines.

Another tube, 5 inches long, and of the ſame diameter as the former, being immerged in ſpirit of wine, the liquor roſe likewiſe near 12 lines; and when immerged in common water, roſe 2 inches 3 lines $\frac{1}{2}$. —— A little piece of a capillary tube being immerged in water, it roſe to the top thereof, and there ſtood.

It appears from all theſe experiments, that common water always riſes the higheſt, tho' we ſee nothing deducible herefrom, which may give any light into the difficulty in queſtion; for as ſpirituous liquors are lighter than water, if their aſcent above the level aroſe from the unequal preſſure of the air, thoſe liquors ſhould riſe higher than water; the contrary of which is found; add that as they are more ſubtile, they ſhould moiſten the parietes of the tubes more readily, and conſequently adhere more plentifully, which would alſo occaſion them to riſe higher.

3dly, I immerged a capillary tube in a veſſel of water, which roſe therein 3 or 4 inches above its level. In this ſtate I ſuſpended the capillary tube, and put the whole under the receiver of an air-pump; reaſoning thus, if it be the unequal preſſure of the air that occaſions the water to aſcend in this capillary tube, when the air is exhauſted from the receiver, the water muſt deſcend and return to a level with that around it;

but if adheſion be the cauſe, no alteration muſt enſue.——The event however proved contrary to this reaſoning; for after the air was exhauſted, the water was ſo far from deſcending in the tube, that it roſe upwards of a line higher. The reaſon whereof may not be far to ſeek; for water being full of a multitude of particles of air, when its ſpring is no longer bent by the preſſure of the upper air, it muſt neceſſarily dilate, and augment the bulk of water. To be the better aſſured of this augmentation of bulk, I put the capillary tube in another tube, ¼ an inch in diameter, which I had filled with water, and marked the height thereof with ink, then exhauſting the air, the water viſibly roſe above its mark; whence it may be inferred, that there are particles enough of air in water to make it ſuſceptible of ſome condenſation.

4thly, But the following experiments determine the queſtion, and ſeem to leave beyond all doubt, that 'tis by the near adheſion of fluids to the inſides of tubes, that makes them riſe above their level; ſo that the other cauſes alledged by ſeveral authors have no ſhare therein.— I run a drop of tallow into a capillary tube melting it to ſuch a degree, till the layer of tallow was extremely thin upon the inſide of the tube, for fear of ſtopping it up, then immerging it in water, the fluid only roſe in the tube to a level, with the other water around it.

This ſingle experiment ſhews, that the unequal preſſure of the air is a fiction; for indeed how can ſuch inequality be conceived, when the aperture of the tube is ſo great with regard to pores, through which the air is able to inſinuate with a great deal of eaſe, and produce the ſame effects, as if it were at liberty, which is eaſily proved; 1ſt, from the experiment of the common barometer, one of whoſe ends has been ſtopped with a hog's bladder; for after making the *vacuum* as uſual, ſo that the preſſure of the adjacent air keeps the mercury ſuſpended 27 or 28 inches high, if a little hole be made in the bladder, with the point of a needle, whoſe diameter is much ſmaller than that of a capillary tube, the air will immediately inſinuate into the tube, and make the mercury fall. 2dly, From what befel me one day in making experiments upon mercury, when the mercury continued falling after the *vacuum* was made, and upon enquiry into the cauſe, I found a little crack in the tube made uſe of, paſting therefore two ſlips of parchment as exactly thereon as I could, I repeated the experiment, but the mercury ſtill deſcended, tho' ſomewhat ſlower indeed, a ſufficient proof of the extreme ſubtilty of air which can inſinuate into the ſmalleſt apertures, and communicate its action thereby.

What confirms the adheſion of water to the inſide of the tube is, that if we only run tallow into a part of the tube below the natural level of the water, the fluid in this caſe will riſe above its level; and if the tallow be only run on one ſide of the tube, the water on that ſide will ſtand at its level, but will riſe on the other ſide above the level. In fine, if a drop of water be let fall along the outſide of the tube, this drop, upon arriving at the extremity thereof, inſtead of falling down, will enter within the tube, but if the tube be ſmeared with tallow, inſtead of entering it will fall directly down.——'Tis evident therefore from the whole, that the cauſe of the water's aſcending above its level in capillary tubes is, that in wetting the parietes of the tube, it adheres to them, and is partly ſuſtained thereby, by which means the other columns of the adjacent water having a greater degree of weight, or preſſing more on the bottom of the veſſel, oblige thoſe over the aperture of the tube to riſe above their pitch.

To conceive how the external columns of water ſhould have more weight than thoſe immediately contiguous to the inſides of the tubes, we advance the following propoſition.——— If a body reſt by one of its extremes on the inequalities of ſome other vertical body, whether by meer oppoſition, or by entering within ſuch inequalities, and be ſuſtained by a power applied to the oppoſite part, the power will be to the weight, or to the effort it makes to deſcend, as the diſtance of the centre of weight of ſuch body from the point of ſupport, is to the diſtance of the power from the ſame point of ſupport.

For ſuppoſe * AB, a vertical ſurface, and ſuppoſe a body as ED, one of whoſe extremes reſts, or is ſuſtained on the point D of the ſame ſurface, and having the point C for its centre of gravity, 'tis evident, that if a power ſuſtained it in the point F, it will not bear the whole weight thereof, ſince we ſuppoſe it ſuſtained in D; but this power will have the ſame *ratio* to the effort the body FD makes to deſcend, as the diſtance DC has to the diſtance FD; for we may ſuppoſe this body as ſuſtained on the middle of a horizontal lever FD, by two powers applied in F and D, now by the laws of equilibrium the power F is to the weight of the body FD, as CD is to FD; wherefore, *&c.*

'Tis eaſy to apply this reaſoning to capillary tubes; for ſuppoſe the veſſel AB full of water, wherein the capillary tube CD is immerged, and ſuppoſe this water divided into columns, compoſed of globules of water, laid one over another as E, 'tis evident, that when the water is entered this tube, all the particles in immediate contact with its parietes, will be partly ſuſtained thereby; now by the laws of fluids, the water will ſtand at a level, provided nothing hinder it, by reaſon all its columns are equally heavy, or preſs the bottom of the veſſel with equal force; but thoſe which touch the inſide of the tube are partly ſuſtained, and conſequently do not act on the bottom of the veſſel with all their force; ſo that the adjacent columns muſt make them aſcend, and this till ſuch time as they make up in height, what they loſe of their weight by adheſion, and thus reſtore the equilibrium.

---

* Plate V. Fig. 7.

From this account, it appears, that fluids, contiguous to the outſides of tubes, ſhould likewiſe riſe to a conſiderable height, which is contrary to experience; but it muſt be noted, that in the inſides of tubes, the particles of the fluids ſuſtain one another, and contribute to their mutual elevation, which does not obtain on the outſide; accordingly we find that water will riſe a little, even in a very large tube.

'Tis evident, that the ſmaller the diameter of a capillary tube is, the higher muſt the water riſe therein; for the force of adheſion is meaſured by the internal ſurface of the tube, and the reſiſtance is meaſured by the weight of the columns of water contained therein; but columns of the ſame height are in a duplicate ratio of the diameter of the tube; whereas the ſurfaces are only in the ſimple ratio of thoſe diameters, conſequently the ſurface of a large tube bears a leſs proportion to the quantity of water it contains, than the ſurface of a ſmall one does to its contents of water, conſequently the force of adheſion is leſs in the large than the ſmall one; wherefore, *&c.*

'Tis evident likewiſe, that in tubes equally, or unequally, inclined, the water muſt always riſe to the ſame height, notwithſtanding its being in greater quantity than when are they vertical; for in inclined tubes, the *momentum* of the preſſure of the water is not meaſured by the whole length of the tube, or by the abſolute weight of the whole column of water in the tube, but by its vertical height, ſince it will only be impelled by the weight of the column of the adjacent water that preſſes freely.

I ſhall here ſubjoin ſome other experiments on the ſame ſubject, which tend to confirm this reaſoning. —— Suppoſe a capillary tube * AB, whoſe inſide is very dry, applying only the end B to the ſurface of a ſtagnant water, it will riſe to C; but if the tube be firſt wetted by paſſing water thro' it, it will riſe higher to D; and if the tube be plunged in the water, it will riſe ſtill higher as to E; if now the tube be taken out of the water, that within it will gradually deſcend, and a little drop will be formed in B; which is the caſe when the height BE is very great, for otherwiſe the water will remain ſuſpended, and not iſſue out; if now the water in B be made to touch a drop of water placed on a plane, the water in the tube will be found to deſcend from E to D, which is the place where it would have ſtood, if only the end B had been made to touch the ſurface of the water. On the contrary, if the water had only roſe to C, and the end B had been made to touch the ſame drop of water on the plain, the water would have roſe to D.

---

* Fig. 8.

The cauſe of all theſe effects depends on the ſame laws of equilibrium; for when the drop in B touches another, it unites thereto by immediate contact, and in this caſe, if the water be in E, being now raiſed too high, it muſt ſink, ſince the whole muſt come to an equilibrium; and if it be too low, as in C, it muſt riſe for the ſame reaſon.

We are now to explain, why ſome bodies are eaſier to wet than others; why different fluids are fitted to wet different bodies; and, laſtly, why certain fluids mixed together, while others will not mix, but always ſeparate from each other.

In order to this, I lay it down as a certain principal, proved in my opinion beyond all poſſibility of contradiction, by father *Mallebranch* and M. *Bernoulli*, 1ſt, That the coheſion and hardneſs of bodies ariſes only from the compreſſion of a ſurrounding fluid; for without admitting a kind of *gluten* in the parts of homogeneous bodies, a name no more intelligible, no fitter to explain the union of certain bodies than of ſympathy, pretending to unite the particles to each other, true philoſophy will allow no other force nor action in bodies, but what ariſes from their motion. 2dly, That this coheſion or hardneſs is ſo much the greater, as the particles of ſuch bodies touch in more ſurface, and leave leſs fluid between them, to reſiſt the action of that without them; ſo that if the reſiſtance be equal to this external action or compreſſion, the particles will not cohere at all; or if the internal fluid reſiſt more than the external one, the particles will fly aſunder; but if the external one act with moſt force, the ſame particles will unite, and this in a greater degree, as their ſurfaces are ſmoother in the ſeveral parts where they join; ſo that if they be ſo ſmooth, as to fit immediately to each other, without leaving any interſtices between them, and conſequently any paſſage for the ambient fluid, they will be compreſſed by the whole force of this fluid; and herein conſiſts the greateſt hardneſs of bodies. After this manner, we may account for the union of two ſmooth bodies, as 2 pieces of glaſs, 2 poliſhed marbles, *&c.* or the union of 2 hollow hemiſpheres of braſs, from whence the air has been exhauſted, which will make ſuch an effort againſt their being diſ-united, that ſeveral horſes will be required to ſeparate them.

The application of this to fluids, which will wet ſome bodies and not others, is eaſy; for when the particles of a fluid have their texture or conſtitution of their little ſurface, ſuch as that they may join moſt immediately to the ſurfaces of bodies, they are contiguous withal, and leave the leaſt fluid between them and the ſurface of ſuch body, they will, in this caſe, adhere, and be, as it were, faſtened and kept together by the preſſure of the ſurrounding fluid; and 'tis for this reaſon, that the drops of water ſuſtained upon the leaves of trees, ſome whereof are extremely ſmooth, do not fall. And from the ſame principal we may explain, why the particles of any fluid unite, and thoſe of a different fluid keep aſunder; for the particles of one and the ſame fluid being all homogeneous, that is, having their ſurfaces nearly ſimilar to each other, when they chance to meet they unite as cloſe as poſſible, leaving very little of the above-mentioned fluid between them, to reſiſt the action of the external one. On the contrary, the particles of different liquors being heterogeneous, that is, having different figures, muſt of conſequence leave a large quantity of the fluid between them, to prevent their joining. Hence, after mixing oil and water, by beating them together a conſiderable time, as the particles of fluids have each of them their motion in all poſſible directions, ſeparately from the reſt, when a particle of oil happens to meet a particle of water, they cannot join cloſe enough to cohere, by reaſon of their figure, and the arrangement of their particles, which makes them to ſlip over one another, inſtead of cloſing; but when a particle of oil meets another particle of oil, their ſurfaces being ſimilar, they approach as near as poſſible, and thus unite by reaſon of the little reſiſtance made from within againſt the action of the fluid without.

Let it not be alledged, that this account tends to deſtroy the fluidity of liquors; for tho' a particle adhere cloſe enough to another to be raiſed, or ſuſtained therewith, on account of the ſmallneſs of its weight, yet it does not adhere ſtrongly enough to reſiſt the impulſe of any other particle ſtriking againſt it, or even the action of the ſubtil fluid, which may inſinuate between them; by purſuing this reaſoning, we may be able to account for the following very curious experiment. —— Wine and oil being mixed together as intimately as poſſible, if we would ſeparate them again, we are only to take two ſlips of whited brown paper, uſed for filturation withal, and dip them ſeparately, the one in wine, and the other in oil, then immerging one end of each in the mixed liquor, and letting the other hang over the edge of the veſſel, we ſhall find the oil drawn off by the paper, which had been moiſtened therewith, and the wine by the other. Now the reaſon is evident, for a particle of wine when it ſtrikes againſt a particle of oil, being unable by reaſon of its figure, to come cloſe enough to expel the ſubtil fluid between them, inſtead of uniting thereto, is neceſſarily repelled; whereas a particle of oil, on the contrary, meeting a particle of wine, approaches thereto cloſely enough to expel the fluid, upon which the ſurrounding fluid compreſſing them without reſiſtance, they remain united together, and ariſe as uſual.

When a greater number of fluids is mixed together, their ſeparation is not effected with ſo much exactneſs; but it appears by the experiment, that water diſengages itſelf the moſt readily from the other liquors it is mixed withal, which may ſerve to account for the great facility wherewith urine ſeparates from the blood, in paſſing thro' the glands of the kidneys, as we ſhall ſhew hereafter.

The ſame principle might likewiſe perhaps ſerve, to explain the different ſecretions made in the body, that is, how the different particles whereof the blood conſiſts, are ſeparated through the glands of the reſpective viſcera, deſtin'd for this office; for all the other ſolutions hereof labour under great difficulties: there are two, among many, which have the moſt air of probability.—— The firſt is, that all the particles of blood are homogeneous, but that the pores of different glands being different, theſe ſerve as ſo many moulds, to give that figure to the particles received within them neceſſary to conſtitute the liquor required. Now 'tis hard to conceive how the chyle, which muſt conſiſt of particles of all the different kinds of foods ſhould be ſo changed, as that ſuch different particles ſhould all become homogeneous in the blood; beſides how ſhall we conceive theſe moulds to act upon liquors which ſtill remain fluid?

The ſecond account ſuppoſes particles of matter in the blood of all kinds of figures, but that the pores of the glands being likewiſe differently figured, anſwerably thereto, let only ſuch particles paſs, as are adapted to them; that is, if a pore be priſmatical, or pyramidal, it will only admit ſuch particles as are of thoſe figures. —— This opinion would carry the more probability, if the particles of blood were of equal bigneſs; but as ſome of them are certainly bigger than others, we do not ſee why a cubic particle, for inſtance, which is ſmaller than a priſmatic pore, ſhould not paſs thro' the ſame, and ſo of others; but if the glands be ſuppoſed to have been moiſtened, from the firſt formation of the body, with the proper liquor, which they are afterwards to ſecrete (as may be ſuppoſed conſiſtantly enough with the modern ſyſtem of generation, which is, that all organized bodies were formed at the time of the creation, and lodged one in another, to be enlarged and unfolded in proceſs of time) it will be eaſy to explain, upon the principle already laid down, how the heterogeneous particles of the blood ſhould ſeparate from each other, and compoſe the ſeveral juices wherewith we find the receptacles of the body filled; for a particle of bile, for inſtance, ſtriking againſt a particle, which is to compoſe ſome other humour, will not adhere thereto, by reaſon of the different texture of their ſurface; but, for the contrary reaſon, will unite to another particle of bile, and both of them go together, to the common receptacle of the bile. After the ſame manner may we account for the nutrition and growth of different plants in the ſame ſoil, on a ſuppoſition, that there are particles of all figures in ſuch ſoil; ſome of them fit for the nutriment of one plant, and ſome of another.

XIX.

XIX. *A continuation of the obſervations on the height of the quickſilver in the barometers, by M.* Amontons *.

Following my firſt views, I mean ſuppoſing that the pores in ſome tubes are more open than in others, and that admitting a paſſage to more parts of air, it is refuſed to none but the thickeſt, which by their weight ſupport the remaining quickſilver in the tube; I took a mean barrel of a gun, of 34 inches $\frac{1}{3}$ in length, I had the breech of it ſoldered, which is properly ſealing it hermetically. When it was cold, I filled the barrel quite full with quickſilver, of which there entered 53 ounces $\frac{1}{2}$. I obſerved, that it held it exactly, not letting any out at any part; after which I prepared to perform the inverſion, but this tube not being tranſparent, the difficulty was to know at what height the quickſilver would ſtop. I at firſt thought to have weighed that which ſhould remain in the tube after the inverſion, that comparing it afterwards by the weight of quickſilver, which quite filled this barrel, I might judge of that height which I ſought. But this not only ſeemed to me troubleſome to execute, but I alſo, for many reaſons, thought it not very certain. For, 1ſt, I was not ſure that this barrel was exactly of the ſame ſize from one end to another: on the contrary, there was ſome reaſon to believe it was not; therefore nothing preciſe could be gained this way. 2dly, Stopping the open end with a finger, in order to hinder the quickſilver in the cup from communicating with that in the tube, it was in a manner impoſſible, but that the quickſilver muſt then be in ſuch balancings, as might give greater or leſs heights than what are true. After having conſidered all this, I ſucceeded in the following manner.

---

* Sept. 2. 1705.

I cauſed the wooden veſſel ABCDE * to be turned, the cavity of which was in the figure of a rectangular cone inverted, and the outſide was a cylinder.

Then having taken the quickſilver out of the barrel, I laid the open end of it in the bottom of the wooden cone, and holding it ſloping as much as poſſible, I poured a little quickſilver all round it, to ſee at what height I ſhould make the the hole FG, which might ſerve to diſcharge the quickſilver of the veſſel ABC, and leave only therein always exactly a ſufficient quantity to hinder the external air from entering at the bottom of the barrel.

After having therefore bored the hole FG, a little ſloping towards E, I ſtopped it with a little wooden ſtopper, which I could take out and put in as I pleaſed; then I filled my iron tube quite full of quickſilver, thruſting into it an iron rod, which I turned about every way, long enough to expel all the little bubbles of air, which might remain adhered to the inward walls of this tube.

Having then poured into the veſſel ABC a quantity of quickſilver, ſufficient for the open end of the tube to dip therein, I put this veſſel into a larger, in order to receive that which might run over by the diſcharge FG during the experiment.

After having dipped the open end of the tube full of quickſilver into that of the veſſel, inſtead of raiſing it perpendicularly, as is uſually done, I held it very ſloping, ſo that in all appearance,

---

* Plate V. Fig. 9.

the *vacuum* muſt not be made in the upper part.

The whole being in this ſtate, I opened the hole G to let out all the ſuperfluous quickſilver, which came out immediately; after which I, by degrees, ſet the tube upright, obſerving exactly the time when I ſaw the quickſilver run again, through the hole G; for that was to ſhow me the point where the *vacuum* was to be begun, which having ſeveral times carefully executed, holding a rule graduated by inches perpendicular to the ſide of the tube, I always found the perpendicular height of the quickſilver above F, at 23 inches, 4 lines, though it was then in other glaſs tubes at 27 inches, 8 lines.

I then left this tube for the experiment: but during the firſt five hours there came out about 13 ounces ¾ weight of quickſilver. In the ſix hours following, there came out 6 ounces ¼ more; and 10 ounces in twelve other hours; and, laſtly, 8 ounces in eight hours more: after which, having quite emptied the tube, I found ſtill therein 4 ounces ¾; ſo that all the quickſilver which remained in the tube, after the inverſion, was 43 ounces, which was to the 53 ½, which filled the tube near the *ratio* of the 27 inches, 8 lines, which the glaſs tube had given, to 34 inches ⅓ length of the iron tube: which would have made me imagine, if I had regard only to the weight of quickſilver, that the *vacuum* was made of the ſame height in the iron as in the glaſs tube. But it is to be obſerved, that the iron tube during the evacuation, was ſo ſloping, that the quickſilver muſt ſtand therein about 6 lines higher, than if it had been perpendicular, and that alſo the ſize of the iron tube in all appearance leſſened near the top;

which I had obſerved only by my eye, and by putting my finger into it before it was ſoldered.

Now though theſe evacuations ſhow, that this tube lets in the air; yet there are many things worth obſervation in this experiment. For firſt, the difference of the 4 inches, 4 lines, which was at firſt found between the heights of the quickſilver, contained at the ſame time in the iron and in the glaſs tube, cannot be imputed to the hole, through which the air muſt have time to inſinuate itſelf into the tube; for, according to the obſervation of the continuance of theſe evacuations, it would have taken near two hours to let in all the air neceſſary to produce this difference, whereas it was inſtantly found.

Secondly, This experiment ſhews alſo, that the parts of quickſilver can by no means paſs through ſuch apertures, as the groſſer parts of air paſs through, when they are both equally loaded. Yet it is known, that quickſilver, when loaded, paſſes through very narrow paſſages, and the ſlowneſs with which the air penetrated into the iron tube, makes me conjecture, that the paſſage through which it paſſed, muſt have been a very little one. In the time of the evacuations, my thermometers were at 55 inches, 9 lines.

To conclude; it does not appear, that a reaſon can be eaſily given for this great difference in the heights of the quickſilver, otherwiſe than by ſuppoſing with me an inequality in the ſize of the parts of air which compoſe the atmoſphere, and that the pores are greater in iron than in glaſs. Yet, as it is not known, whether the ſame thing would happen in other tubes of iron, I dare not conclude any thing thereupon, and look on this only as a preliminary experiment, which precedes thoſe which are to confirm or explain it:

For, in ſhort, perhaps the ruſt, which is conſiderable in the inſide of this tube, retains many particles of air, which hinder the *vacuum* in this tube, from being made ſo perfect as in thoſe of glaſs; which is yet what I can hardly believe, conſidering the care I took to get it out; and I cannot imagine, that a ſufficient quantity ſhould remain to produce ſuch a conſiderable difference, independant of the pores of the metal.

I have ſaid, in my laſt memoir, that ſpirit of wine perhaps only occaſioned a leſs height in tubes which had been waſhed with it, becauſe it made them cleaner, and hindered the droſs of the quickſilver from fixing to them.

On this occaſion, it would not be from the purpoſe for me to relate ſome experiments thereupon, which have ſhewn me, that the cleaneſt quickſilver, which has been long ſhaken in a very clean glaſs, ſullies and obſcures it very conſiderably. For having often carried in my pocket little bottles of quickſilver, ſome of which were even ſealed hermetically, having carried them for a year or longer, I always found the bottle not only very dirty, but the quickſilver partly reduced to a black duſt, like a pounded coal, as the company may have obſerved in that which I have long made uſe of, in form of one of thoſe levels, called *ball levels*, in which the oppoſite ſides are ſeldom parallel; though it is neceſſary to make the uſe of it certain, which is not ſo in this caſe.

But to return to our ſubject, it is therefore very poſſible, that the matter which paſſes through the pores of the glaſs, which has hitherto been thought to be only that of light, finds more or leſs hinderance to its paſſage, according as the entrance of theſe pores is more or leſs entangled with a foreign matter, ſuch as may be the droſs and leaden part of the quickſilver, or of ſome other matter floating in the air, capable of producing ſuch an effect; as it would happen to a fine ſieve, expoſed for ſome time to the ſmoak: for the ſoot which would cleave to it, might ſtop the holes; ſo that what before paſſed eaſily through it, could no longer paſs at all, or with difficulty; and as by waſhing this ſieve, it might be recovered to its firſt ſtate. In like manner alſo, ſpirit of wine, or other liquors, may take off that kind of ſoot which hinders that paſſage to the little parts of air, which the greatneſs of the pores of the glaſs would perhaps otherwiſe allow them.

XX. *New reflections on the rules for the condenſation of the air, by M.* Caſſini *the ſon* †.

The perpendicular height of *Mount d'Or*, above the level of the ſea, was meaſured to be 1040 toiſes, by the obſervations which we made on it, to determine the triangles of the meridian. The height of the quickſilver was there obſerved, by F. *Sebaſtien*, 22 inches, 2 lines, *June* 8. It was then at *Paris*, in the tower of the hall of the obſervatory, 22 inches, 9 lines ½. There was a difference therefore of 5 inches, 7 lines ½, to which, if we add 4 lines for the difference, which agrees with the height of the obſervatory above the level of the ſea, we ſhall have for 1040 toiſes height of *Mount d'Or* above this level, 5 inches, 11 lines ½ falling of the quickſilver, which is in the proportion of 14 toiſes, 3 feet, and ſome inches for each line, one with another. Accordng to the table *, which I have drawn up, according to M. *Mariotte*'s rules, allowing with him for the firſt line of quickſilver, which anſwers to the level of the ſea, 10 toiſes, 3 feet, we have for the line which anſwers to 6 inches diminution of quickſilver, which is nearly that which is found upon *Mount d'Or*, 13 toiſes, 2 feet, 2 inches, 2 lines leſs, than what is found for each line of quickſilver, even though we ſhould ſuppoſe no augmentation cauſed by the dilatation of the air.

---

† Sept. 2, 1705.

Continuing to compare the table, drawn by the rules with the experiments, we ſee that at 6 inches diminution of quickſilver, the height of the air above the ſurface of the ſea, ſhould be 852 toiſes inſtead of 1040, as has been found by obſervation, and that at the height of 1044 toiſes above the level of the ſea, which is nearly that of *Mount d'Or*, there ought to have been found a diminution of 7 inches, 2 lines of quickſilver; that is, above 14 lines more than has been found by F. *Sebaſtien*'s experiment, compared with that which was made at the ſame time at the obſervatory.

This difference is ſo conſiderable, that it cannot be aſcribed to any error that could have been made in meaſuring the height of the mountains, nor to the different temperature of the air, which could have made a different variation in the height of the quickſilver at *Paris*; and at *Mount d'Or* we have found, that the variations in the height of the quickſilver uſually happened at the ſame time; and if there were ſome differences, they never were near ſo conſiderable.

The obſervation, which F. *Sebaſtien* made at *Clermont*, gives us room to examine with more exactneſs the experiment, which M. *Perier* made on the *Puy de Domme*; and which M. *Mariotte* makes uſe of for the confirmation of his rules. *June* 10, 1705, F. *Sebaſtien* obſerved near the *Minims*, which is the very place where M. *Perier* made his experiments, the height of the quickſilver to be 26 inches, 6 lines. By the obſervations made at *Paris* before and after, it was 27 inches, 10 lines. The difference is 1 inch, 4 lines, which agrees with the height of *Clermont* above the obſervatory; to which if we add 4 lines, for the height of the obſervatory above the level of the ſea, we have 1 inch, 8 lines, for the height of *Clermont* above the level of the ſea. If we add to this difference 3 inches, 1 line $\frac{1}{2}$, which is that which M. *Perier* found between the *Minims* of *Clermont*, and the top of the *Puy de Domme*, we ſhall have for 812 toiſes, the perpendicular height of the *Puy de Domme*, above the level of the ſea, determined by our obſervations, a diminution of quickſilver of 4 inches, 9 lines $\frac{1}{2}$. According to M. *Mariotte*'s rules, the height of this mountain ought to be but 663 toiſes; and at the height of 812 toiſes, we ought to have found 5 inches, 9 lines diminution of quickſilver; that is, 11 lines $\frac{1}{2}$ more than we have found by the experiments. We ſhall find a ſtill greater difference, if in the room of our obſervations, we uſe thoſe which M. *de la Hire* has made at the obſervatory, which make the height of the quickſilver above a line lower than what we have obſerved. Here are ſeveral obſervations, made by various perſons at different times, which all vary from the rules which M. *Mariotte* has eſtabliſhed for the condenſation of the air; we ſee alſo that his rules cannot anſwer exactly to experiments, whereas, according to the remarks which M. *Maraldi* read lately to the academy, there is but one ſingle obſervation, which varies about 4 lines from the rule, which he has laid down.

---

* See above, pag. 251.

XXI. *An experiment on the quantity of heat reflected to us by the moon, by M.* de la Hire *jun.* [†]; *tranſlated by Mr.* Chambers.

'Tis notorious that ſeveral perſons attribute various qualities to the moon, without any reaſon for the ſame founded on experiments. I ſhall not here enter upon a detail of thoſe qualities, as having obſerved, that moſt of thoſe who give into the practice, differ in their ſentiments, that which it ſhould ſeem to have the moſt title to is heat, ſince its light is only that of the ſun reflected, which every body will allow muſt occaſion heat: however, as no experiments, that I know of, have been made, either to ſupport or over-turn the reaſon upon which this quality is given it, I made the following one, with all the accuracy I was capable of.

In the month of *October*, 1705, the moon being in the meridian on the day of her oppoſition, and the ſky very clear, I expoſed the burning concave, of 35 inches diameter, kept in the obſervatory, to her body, and about the *focus* placed the ball of an air thermometer of M. *Amontons*'s, which is the moſt accurate of any we have known. The whole being ſo diſpoſed, as that the ball, which is 2 inches in diameter, received all the rays gathered into the *focus* upon its whole ſurface; hereupon examining the height of the mercury in the tube, after it had ſtood ſome time, I found it nothing different from what it was

---

[*] Nov. 28, 1705.

before, notwithſtanding that the rays were collected into a ſpace 306 times leſs than they naturally poſſeſs, and conſequently ſhould have increaſed the apparent heat of the moon 306 times.

Now, if ſuch an experiment as this, where the rays of the moon are not crowded into the 306th part of their uſual room, but made to croſs and interſect each other, which muſt conſiderably increaſe their effect, as is found by expoſing the ſame concave to the ſun, give us no ſigns of heat, we may conclude it incapable of making any ſenſible impreſſion upon us.

A

TABLE

OF THE

PAPERS contained in the ABRIDGMENT of the HISTORY and MEMOIRS of the ROYAL ACADEMY of SCIENCES at PARIS, for the Year MDCCVI.

In the HISTORY.

I. *ON an irregularity of ſome barometers.*
II. *On the declination of the needle.*
III. *Why a glaſs veſſel filled with water, or quick-ſilver, breaks before the fire in winter, but not if it is filled with ſpirit of wine.*
IV. *Of ſome dogs almoſt ſtarved to death.*
V. *Of a man continuing fifteen days alive under the ground.*
VI. *Of an extraordinary ſtone brought from* Germany.
VII. *Of an accidental mixture of drugs, which produced a ſmell of musk.*
VIII. *Of the* formica-vulpes.
IX. *On the cryſtalline humour of a ſerpent's eye.*
X. *Of a worm, which ſeems to have two heads.*
XI. *Of an inſect fiſh.*
XII. *Of ſome extraneous foſſils, found in* Germany.
XIII. *Of an egg within an egg.*
XIV. *Of the ſtomach of a dog found in the* thorax.
XV. *Some inventions of M.* Deleſme.

In the MEMOIRS.

I. *Obſervations on the quantity of rain-water, which fell at the obſervatory, during the* *year* 1705, *and on the height of the thermometer and barometer.*

II. *Obſervations on the rain and wind, made in the year* 1705, *at the caſtle of* Pontbriand, *ſituated two leagues from* St. Malo, *in* Bretagne.

III. *Other obſervations on the rain, which fell during the year* 1705, *at* Lyons, *communicated to M.* Caſſini *by F.* Fulchiron, *a jeſuit.*

IV. *Obſervations on the barometer and thermometer, made at different places, during the* year 1705, *by M.* Maraldi.

V. *Remarks on the bivalve ſhells, and firſt concerning muſcles, by M.* Poupart.

VI. *An extract of the obſervations made in* December, 1705, *by M.* Bianchini, *on ſome fires which are ſeen upon one of the* Apennine *mountains, by M.* Caſſini *the ſon.*

VII. *A diſſertation on the barometers and thermometers, by M.* de la Hire *the ſon.*

AN
ABRIDGMENT
OF THE
PHILOSOPHICAL DISCOVERIES and OBSERVATIONS in the HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1706.

I. *On an irregularity of ſome barometers; tranſlated by Mr.* Chambers.

SO many ſeveral opinions being ſtarted in the academy, touching the phænomenon of the chancellor's barometer, the concluſion was, that new experiments ſhould be made. M. *Maraldi* accordingly had made ſeveral; and they all ſeemed to confirm the ſentiment of M. *Homberg*, who apprehended that the barometer above-mentioned ſtood lower than the reſt, by reaſon, after filling it with mercury, it had been waſhed in ſpirit of wine, ſome particles of this remaining in the pores of the glaſs, might, when the *vacuum* was made, become extremely rarefied, and thus depreſs the mercury, either of themſelves, or by means of the air contained in them, and diſengaged by their rarefaction.

The reſult of M. *Maraldi*'s experiments is as follows.—— After waſhing the inſide of a tube with ſpirit of wine, and wiping it ſeveral times with different pieces of linnen, the mercury uſually ſtands at a leſs height than before; and in different experiments, the difference of heights varies from ½ an inch to an inch ½.

If the tube be filled immediately after waſhing it, the mercury will ſtand lower than if the tube had been filled ſome hours later.

If a tube have been waſhed with ſpirit of wine, the mercury will ſtand lower in it, than if the ſame tube had been waſhed with brandy; and if it have been waſhed in brandy, the mercury will ſtand lower, than if it had been waſhed in water.

If the tubes waſhed with theſe ſeveral liquors be afterwards well wiped, and dried, the mercury will ſtand at the ſame height as before they were waſhed.

A tube waſhed with ſpirit of wine, will be ſufficiently dried by ſtanding ſeveral days in the air, provided it be not moiſt.

Tho' the outſide of a tube be waſhed, and rubbed ever ſo much with ſpirit of wine, the mercury will maintain its uſual height.

In a barometer, which had two cracks at its upper end, the mercury did not ſink for two months; that is, it only ſunk as in other barometers.

Upon making barometers with ſeveral different tubes, none of which appeared moiſt, the mercury ſtood at different heights; the greateſt difference being $\frac{1}{6}$ of an inch; but upon drying the tubes wherein it was loweſt, it roſe to the ſame height as in the reſt.

From all this, it is eaſy to conclude what care and precaution is to be taken in making a good barometer.—— As to the theory, all that can be alledged is, either that the little drops of the fluid, wherewith the inſide of the tube had been moiſtened, became extremely rarefied in the *vacuum*, ſo as to depreſs the mercury, or that the air incloſed in ſuch fluid, diſengaging itſelf therefrom

from by this rarefaction, were the cauſe of depreſſing the ſame.

The firſt opinion is the leaſt probable, by reaſon if the ſpirit of wine ſunk the mercury by its own means, it would ſink it leſs than brandy, as being lighter than brandy; and brandy, being lighter than water, would likewiſe ſink it leſs than water, which is contrary to experience. It would follow therefore, that, agreeably to the ſecond opinion, there is either more air incloſed in ſpirit of wine, than in brandy; or that it diſengages itſelf more readily from the former than the latter; and the ſame will hold in the compariſon of brandy with water.

But the great difficulty is ſtarted by M. *Amontons*, ſtill remains; and till this be removed, the hypotheſis hitherto explained, how plauſible ſoever, will only be an hypotheſis.

II. *On the declination of the needle; tranſlated by Mr.* Chambers.

That noble ſyſtem of Dr. *Halley* concerning the variation of the load-ſtone, which has been laid down in the hiſtory of 1701 *, and verified by the academy, in the hiſtory of 1705, is now further confirmed by M. *Deliſle*, who, having an exact journal of a voyage to *Guinea* and *America*, made by M. *de Marchais* in the year 1704, 1705, and 1706, has been at the pains of comparing all the obſervations relating to the variation, with Dr. *Halley*'s chart; that chart having been made for the year 1700, the declinations are not to be found the ſame in the ſucceeding years, as laid down by him, but different a little therefrom; and this more or leſs according to the length of time, which diverſity, provided it agree with Dr. *Halley*'s ſyſtem, is a full confirmation thereof.

---

* Vol. I. Page 245 of this abridgment.

Now this is what M. *de Liſle* has found; the curve line of no variation, drawn by Dr. *Halley* round the globe of earth, only differs from that produced by M. *Marchais*'s journal, by its being about half a degree more to the weſtward, as accordingly it had always been expected to find a motion in this line.

From this term, the declinations, obſerved by M. *Marchais* do all increaſe towards the eaſt, and diminiſh towards the weſt, with regard to thoſe in Dr. *Halley*'s chart; and the greateſt difference, which indeed only occurs once or twice, amounts but to about 2°— in 4 or 5 years. This is a new inſtance of what had been obſerved before, *viz.* That the declination does not vary equally and uniformly in all parts of the earth.

III. *Why a glaſs veſſel filled with water, or quickſilver, breaks before the fire in winter, but not if it is filled with ſpirit of wine.*

M. *Homberg* ſays, that a glaſs veſſel put before the fire in winter breaks if filled with water, and more eaſily if filled with quickſilver; but not at all if filled with ſpirit of wine. The reaſon of which he imagines is, that the matter of light paſſing with difficulty through water or quickſilver, and conſequently partly ſtopped by this obſtacle, gathers into the pores of the glaſs in too great a quantity, where the fire continually drives it, and dilating theſe pores too much, forces the ſpring of the glaſs, and thereby breaks it; whereas had it met with ſpirit of wine in the ſame veſſel, which is more homogeneous to it, and which it eaſily penetrates, it would not have had occaſion to exerciſe this violence. The experiment muſt be made in winter, becauſe the veſſels which then paſs from a cold air to great heat, are more diſpoſed to break; but the fire muſt not be ſo great, or they ſo near it, as to break on this account alone. M. *Homberg*, much in the ſame manner explains, why an empty veſſel, unſtopped, being ſuddenly warmed before the fire, commonly breaks if it is thick; but not if it is thin. The thickneſs makes the matter of light dilate the pores of the ſurface towards the fire, much more than the inward ſurface; and from this inequality of dilatation, all the reſt evidently follows.

IV. *Of ſome dogs almoſt ſtarved to death.*

A *Daniſh* bitch, ready to whelp, having been forgot, and ſhut up in a chamber of a country-houſe at the family's returning to *Paris*, was, at the end of 41 days, found alive, lying on a bed, but could not ſtand, and without any ſigns of madneſs. No remains of its puppies, or excrements, were to be ſeen, ſhe muſt have fed on thoſe, and poſſibly on her milk alſo, and even on part of the dimity of a quilt, which ſhe had torn, and of the wool in it, which ſhe had tumbled about. Food was given her, and ſhe began to recover from her extreme weakneſs, when M. *l'Abbé Galois* related this hiſtory.

On this occaſion, M. *du Hamel* mentioned another bitch, which was 6 weeks without eating, except the ſtraw of a chair, which was in the place where ſhe was confined; ſhe had alſo drank ſome water, and lived very long after.

V. *Of a man continuing fifteen days alive under the ground.*

M. *Maraldi* alſo relates on this ſubject, that in an earthquake at *Naples*, a young man continued 15 whole days under ſome ruins, and was not ſtarved to death.

VI. *Of an extraordinary ſtone brought from* Germany.

M. *Geoffroy* ſhewed a ſtone brought from *Germany*, he knows not from what part thereof. It is marbled, very ſoft to the touch, and in a manner fat and ſaponaceous. It is like ſoft marble, or petrified ſoap. It was thought to be a dry, hardened, ſlimy earth. M. *Homberg* ſays, its nature conſiſts in having a finer grain than marble, tho' it weighs leſs, having greater pores. He adds, as a proof of the fineneſs of its grain, that being pounded and diſſolved in water, it thickens it, which marble does not. Its effects are very near the ſame as ſoap. M. *de la Hire* ſays, that at *Montmartre* there is ſuch a ſtone among the banks of ſand.

VII. *Of an accidental mixture of drugs, which produced a ſmell of musk.*

M. *Lemery* having bought of a druggiſt half a pound of *galbanum*, as much *ſagapenum*, as much *bitumen judaicum*, and 4 ounces of *opopanax*, put all theſe drugs in his pocket, each wrapped up ſeparately in little bags, except the *ſagapenum* and *opopanax*, which were together, he was much ſurpriſed, when he returned home, that every body thought he ſmelt of muſk; for every one of theſe drugs ſeparate has a ſtinking and very penetrating ſmell, except the *bitumen judaicum*, which, however, has no ſmell like muſk; and theſe very drugs are made uſe of in medicines againſt the vapours, a diſeaſe which may be occaſioned by muſk, and ſuch like ſmells. He examined all the bags after one another; they were all new, none had been uſed to wrap up muſk, or ſmelled of it, but only of the drug which had been put therein. He brought them all near one another again, and they produced a ſmell of musk. M. *Lemery*'s cloaths were pretty ſtrongly perfumed with it, which laſted till next day.

VIII. *Of the* formica-vulpes.

M. *Poupart* hath in the memoirs of 1704 *, given the hiſtory of the *formica-leo*, we preſuppoſe this for the underſtanding of what follows. A friend of M. *Carré*'s, ſeeking for theſe inſects in the country, found many of thoſe holes which they ſo cunningly make; but moſt of them without a *formica-leo*, which made him think, they had been a prey to ſome animal, who were greater *lions* than they. He was ſoon undeceived, obſerving at the bottom of theſe holes little worms, of about 6 lines long, and ½ a line broad. He took ſome of them and put them in ſand, where he ſaw them make their holes in the manner of the *formica-leo*. He gave them ants, which the *formica-leo* ſo much loves, and they greedily ſeized them, covering them with half their body, for the other part remains buried in the ſand. As they have not ſo much ſtrength as the *formica-leo*, their prey often eſcapes from them, and to recover it, they make uſe of the ſame ſtratagem, they build their pit more ſloping, which makes the animal fall back. The *formica-leo* likes them very well when they are given to him; but this is no wonder, for they like to eat one another. Theſe worms change into an inſect, very like a gnat, except that it is longer and thicker. The obſerver calls them, *formica-vulpes*, to diſtinguiſh them from the *formica-leo*; and deſcribe their cunning.

---

* Page 161 of this volume.

IX. *On the cryſtalline humour of a ſerpent's eye.*

The ſame friend of M. *Carré*'s examining the cryſtalline of a ſerpent's eye, which was a line in diameter, found it, even with the microſcope, to be perfectly ſpherical. As it reſembles a blown *lens*, he had a mind to view objects through it, and found that it magnified them exceedingly, full as much as ſuch a *lens* of glaſs would have done, but it was not ſo tranſparent; probably by reaſon of the membrane which covers the cryſtalline. Hence it is certain, that theſe animals muſt ſee objects incomparably greater than we ſee them.

X. *Of a worm, which ſeems to have two heads.*

The ſame obſerver of nature met by chance a worm, 2 inches long, and 1 line broad, and $\frac{1}{4}$ thick, of a pretty deep yellow, like ear-wigs, and 80 legs on each ſide. The head and tail differ ſo little in their figure, that it cannot be conjectured, which of the two extremities is the head. Nor can it be diſtinguiſhed by the animal's walking; for if we ſtop it, it does not turn on one ſide like others, but returns ſhort upon its ſteps, going backwards; ſo that the part, which in its firſt motion was behind, becomes foremoſt in the ſecond, and theſe two motions are with equal facility. Perhaps, this inſect hath two heads, and two brains, as others have many lungs. However, its extremities terminate in a point, with two little horns, like to its legs, and about 1 line long. It is ſprightly and very nimble; and the order with which it ſucceſſively moves its 160 legs is admirable.

The philoſopher, who obſerved it, cut it in two equal parts, each of which conſequently had 80 legs, they both walked as nimbly as the whole animal; they endeavoured to hide themſelves in ſome hole, and the obſerver having put water in their way, each of them went a little into it, but ſoon got out again. He again cut each part in two, and ſtill all four walked, but more ſlowly; it often made contorſions, like the tails of ſerpents when they are cut off. The ſeparated parts did not endeavour to join; when they were put together, they ſtuck a little by means of a viſcous juice which iſſued from their wounds, but they did not agree in their motion.

XI. *Of an inſect fiſh.*

This philoſopher has alſo found an inſect fiſh, which transforms into an adder-bolt. When it is in the water, it is near 2 inches long, of which the tail is two thirds, and is 4 lines broad in the middle, terminating in a point. It is flat underneath, and round above. In the other third of this animal's length, is its head and ſix legs. The adder-bolt, which comes out of it, is one of thoſe which fly about ſtanding waters, where they lay their eggs. Here is an animal, which from a fiſh, becomes a bird, different in all likelihood from thoſe mentioned by M. *Poupart* in the memoirs of 1704* ; perhaps, by much obſervation, it may be found, that this change of habitation and of element is pretty common.

XII. *Of ſome extraneous foſſils found in* Germany.

What has been related in the hiſtory of 1703†, of ſtones taken out of the *Veroneſe*, which incloſed plants and dry fiſhes, was confirmed by M. *Leibnitz*. He ſays, that in the country of *Brunſwick* near *Oſteroda*, in the county of *Manſfield* near *Eiſlebe*, and in many other parts of *Germany*, veins of ſlate are found very near horizontal, whereon are very exact and compleat repreſentations of many ſorts of fiſhes or plants, which appear of their natural length and breadth, but without any thickneſs. Theſe lines are often deſcribed on a mixture of copper, which alſo contains ſilver. Some of theſe plants are not now known in thoſe countries ; but they may be found among the figures of *Indian* plants.

M. *Leibnitz* conceives, that a kind of earth has covered lakes and meadows, and buried therein fiſhes and plants ; or that ſome muddy water, filled with earth, has covered them, or carried them away. This earth has ſince hardened into ſlate, and the length of time, or ſome other cauſe, has deſtroyed the delicate ſubſtance of the fiſh or plant, near in the ſame manner as the bodies of flies, or ants, which are incloſed in yellow amber, have been waſted, whereof nothing palpable remains, but ſimple delineations only. The matter of the fiſh or of the plant being conſumed, has left its form impreſſed on the ſlate, by means of the cavity which remained therein; and this cavity was at laſt filled up with a metallic matter, whether a ſubterranous fire baking the earth into a ſlate, has expelled the metal that was mixed with it, or that a metallic vapour penetrating the ſlate, has fixed itſelf in theſe cavities. M. *Leibnitz* adds, that this effect may be imitated by a pretty curious operation. Take a ſpider, or any other ſuch-like animal, and wrap it up in clay, leaving a hole open from the outſide into the cavity. Put the maſs into the fire to harden it; the ſubſtance of the animal will be reduced to aſhes, which may be waſhed out by ſome liquor. After which pour into the hole melted ſilver, which being cold, within the maſs will be found the figure of the animal, well enough repreſented in ſilver.

---

* Page 171 of this volume. † Page 13 of the ſame.

Many authors have called theſe ſorts of repreſentations of fiſhes, or of plants in ſtones, *ſports of nature*, but that is only a meer poetick idea, which does not ſuit with ſuch a philoſopher as M. *Leibnitz.* If nature ſported, it would be with more freedom; ſhe would not confine herſelf to expreſs ſo exactly the ſmalleſt features of the original; and what is more remarkable, preſerve ſuch true dimenſions. When this exactneſs is not found, it may be ſport; that is, diſpoſitions which are in ſome ſort accidental. It is true, that a repreſentation of an *Indian* plant in a *German* ſtone, ſeems at firſt contrary to the ſyſtem of M. *Leibnitz.* But that the plant repreſented ſhould be found in the *Indies*, favours much the opinion, that there is no wantonneſs therein. It is eaſy to imagine many accidents, whereby a plant may have been brought from the *Indies* into *Germany*, even in thoſe times when there was no commerce between theſe countries by navigation; and it appears by ſeveral marks, that there has been great changes in nature on the ſurface of the earth. M. *Leibnitz* thinks the ſea once almoſt covered the whole, and that afterwards a great part of its waters made a paſſage into thoſe hollow abyſſes which are within our globe. From thence proceeds the ſhells of the mountains. But the whole of this ſubject deſerves a more ample diſcuſſion.

XIII. *Of an egg within an egg.*

M. *Mery* ſhewed a hen's egg boiled, in the white of which was incloſed another little egg, covered with its ſhell and inward membrane, and filled with a white matter, without yolk. As this little egg had been given when boiled to M. *Mery*, he could not obſerve whether it had a ſperm.

XIV. *Of the ſtomach of a dog found in the thorax.*

M. *Littre* diſſecting a dog, was much ſurprized to find his ſtomach in the thorax, and placed above the diaphragm. Inſtead of the hole through which the *œſophagus* croſſes the diaphragm to go into the ſtomach, there was a great fiſſure, the lips of which were cicatrized, and ſeemed to have been a long time ſo, and inſtead of the *œſophagus*, the gut duodenum went through this hole. As it is always faſtened to the inferior orifice of the ſtomach, it followed it into the cavity of the *thorax*, which it could not do, without being ſtretched and flatted. M. *Littre* would ſee whether the ſtomach could paſs thro' tht fiſſure in the diaphragm, but it was too little; and after he had made an inciſion therein, the ſtomach fell into its natural place, and the *æſophagus* was long enough not to hinder it, which ſhews that the ſtomach had at firſt been in its right ſituation, but ſome violent accident had removed it by a rent or ſlit in the diaphragm, which afterwards ſhrunk when it cicatrized.

But what could this accident be? M. *Littre* gueſſes two, either an extraordinary convulſion of the *æſophagus*, which by contracting had drawn the ſtomach towards it; or an extreme contraction of the diaphragm, and at the ſame time of the muſcles of the belly; for theſe forces being oppoſite, the ſtomach, thruſt downward by the diaphragm, and upward by the muſcles of the belly, muſt go upward, even tearing the diaphragm, whoſe reſiſtance is naturally leſs than the force of the muſcles, which act againſt it. It may alſo be ſuppoſed, that this ſtomach was then full of ſolid food, which muſt have added ſtrength to its action againſt the diaphragm.

However, this ſtomach being now not placed between the diaphragm and the muſcles of the belly, theſe muſcles could no longer, as uſual, concur with it by their contraction in pounding the food, and driving it into the guts. They muſt therefore remain too long in the ſtomach, and thereby contract ſome vice. Beſides the *duodenum* being ſtretched and flatted, did not give a paſſage free enough to the digeſted food. In ſhort, the heart and lungs were much diſordered in their motion by this ſtomach, which took up its lodging in the *thorax*. From all which it may be eaſily concluded, was it no otherwiſe

known;

AN ABRIDGMENT OF THE

PHILOSOPHICAL MEMOIRS of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1706.

I. *Obſervations on the quantity of rain-water, which fell at the obſervatory during the year* 1705, *and on the height of the thermometer and barometer, by M.* de la Hire*.

I Made obſervations on the quantity of rain-water which fell at the obſervatory throughout the year 1705, in the ſame manner as on the preceeding years. I found the height of rain-water in the month of

	Lines.		*Lines*
Jan.	5 ¾	July	2 ¾
Feb.	8	Aug.	19
Mar.	7 ⅛	Sept.	16 ⅛
Apr.	23 ⅜	Octob.	27 ⅞
May	4 ½	Nov.	13 ¼
June	15 ½	Dec.	23 ½

The quantity of water in height has therefore been this year 166 lines ¾, or 13 inches, 10 lines ¾, which is not much more than two thirds of what commonly falls; which, by the compariſon for many years, I have eſteemed to be 19 inches. I have not met with ſo great a drought as this, during the many years I have made theſe obſervations: yet has the harveſt been plentiful enough, which may be attributed to the great rains in the month of *April*, which ſufficiently ſupplied the earth with moiſture againſt the following drought.

---

* June 9, 1706.

The three ſummer months, which commonly furniſh as much water as all the reſt of the year, by reaſon of ſtorms and pouring rains, gave but 37 lines $\frac{1}{4}$, and *July* furniſhed but 3 lines. Nor was it the great rains of this ſeaſon, which contributed to the fruitfulneſs of the earth; for they riſe again in vapours almoſt as ſoon as they fall, and part of them run off without penetrating deep.

The winds were in *January* very changeable. All the beginning of *February* they were eaſterly, ſometimes inclining to the N. and ſometimes to the S. and at the end towards the W. In *March*, the wind was almoſt always at the E. ſometimes paſſing to the N. and ſometimes to the S. In *April*, the governing wind was about S. W. In *May*, the wind reigned in the N. ſometimes varying towards the W. In the firſt half of *June*, the wind was as in *May*, and without rain; but the 14th, it began to rain till the 17th with a N. wind afterwards N.W. and S.W. and there fell 11 lines of water, and the 22d, 4 lines, with a little ſtorm: the reſt of the month, the wind was N. and N. E. n *July*, the governing wind was N. and in this month there was very little rain. In *Auguſt*, the wind was almoſt always W. and S. W. In *September*, the governing wind was W. varying a little to the S. and N. In *October*, the wind was often N. inclining ſometimes to the E. and to the W. In the firſt half of *November*, the wind was N. and N. E. and in the other half S. W. and N. W. In *December*, the governing wind was very high at S. and S. W.

with very great violence, and a ſort of hurricanes riſing two or three times. The 3d of this month in the evening, the wind was very high in the S. with thunder; which, in this country, ſeldom happens in the winter.

There has been no ſnow this year.

The thermometer with ſpirit of wine ſealed hermetically, which I make uſe of to meaſure the cold and heat, indicated, that the cold in the beginning of the year was not conſiderable, for this thermometer fell only to the 25th degree *February* 2, and to 30 degrees *November* 13, when it froze pretty hard, and immediately it roſe again to 40 degrees. It always begins to freeze in the country, when it is fallen to 32. Its mean ſtate, as it is in the bottom of the caves of the obſervatory, is 48 degrees. Theſe caves are 14 toiſes under ground, and very near the level of the river, when it is at a mean height. In the morning, at the ſun-riſing, the greateſt height of the thermometer was at 65 degrees ½, *Aug.* 18; but near 3 in the afternoon, when the air of the day was hotteſt, the thermometer roſe at the end of *July*, and the beginning of *Auguſt*, to 75 degrees; and *Auguſt* 6, it was at 80 degrees, tho' it is ſhaded and expoſed to the air in the open tower of the obſervatory, which ſhewed an exceſſive heat; and I queſtion, whether it has ever been greater in this country. And moſt of the thermometers with ſpirit of wine broke, the liquor contained therein having not had room enough to riſe in the top of the tube, which did not happen to mine, having made it very long, that I might expoſe it to the ſun.

It ought to be obſerved, that the greateſt heat of the afternoon does not always anſwer to that in the morning, through ſeveral particular reaſons.

By theſe obſervations, it is alſo ſeen, that the heat has this year been in a greater proportion than the cold; for the thermometer exceeded its mean ſtate in the heat 32 degrees, and in the cold it fell below it only 23 degrees.

Theſe are the obſervations on the weight of the air, which the barometer has ſhewn us. In that which I commonly make uſe of, which is one of thoſe called ſimple, and is always placed at the height of the great hall of the obſervatory, the quickſilver roſe higheſt to 28 inches, 3 lines ⅓ *Feb.* 28, with a ſmall N. N. E. wind, and it fell at loweſt to 26 inches, 7 lines ⅚ *Dec.* 20, with a W. S. W. wind; thus the difference of a height between the loweſt and higheſt was 1 inch, 7 lines ½, much as uſual.

The great elevation of the quickſilver in the tube of the barometer ſeems commonly to be only when the wind is northward; and on the contrary, it almoſt always falls loweſt when the wind is ſoutherly, and when it is violent and tempeſtuous; yet there are particular cauſes which may make the air heavier or lighter, tho' the wind is neither northward or ſouthward. Therefore the obſervations of the barometer are not to be too much depended upon concerning what the weather will be.

I ſhall here obſerve by the way, that there are ſome barometers in which the quickſilver riſes much higher than in others, at the ſame time and at the ſame place, though they are made with great care. In that which I commonly make uſe of, the obſervations which I have now related, the quickſilver is always 3 lines ½ lower than in another of mine, which is that wherein the light was firſt obſerved in the vacuum at the top of the tube, by agitating the quickſilver,

though

though in the other there appears to be ſome alſo. But if in the thermometer, wherein the quickſilver does not riſe ſo high, the groſs air has not been ſo exactly evacuated as in the other, by reaſon perhaps of the great length of its tube, it will follow, that it is not neceſſary that the quickſilver ſhould be exactly cleanſed from air to make the light appear in the *vacuum*. In ſhort, after all the experiments we have made on barometers with different quickſilver in the ſame tube, and different tubes with the ſame quickſilver, it ſeems neceſſary to believe, that the different heights of the quickſilver in the tubes of barometers, proceed only from the nature of the glaſs, whoſe pores are not equally cloſe, and that the air is not only compoſed of two different matters, one of which is altogether groſs, and the other ſubtile; but that the particles of the groſs matter differing in their ſize, even till they become ſubtile, the pores of ſome glaſſes ſuffers this leſs groſs-matter to paſs, which by its weight makes the quickſilver in the tube fall a little. This hypotheſis ſeems confirmed by one of the laſt experiments, made by M. *Amontons* at the academy, with the barrel of a gun well ſoldered at one end, inſtead of a glaſs tube, wherein it was obſerved, that the quickſilver ſtood much lower than in a common glaſs, perhaps becauſe the pores of iron are much greater that thoſe of glaſs.

I obſerved the declination of the needle 9° 35′ towards the W. the laſt day of *Dec.* 1705, with the ſame needle and in the ſame place, where I have obſerved it many years, and with the ſame circumſtances, as I have related in other memoirs.

II. *Obſervations on the rain and wind, made in the year* 1705, *at the caſtle of* Pontbriand, *ſituated two leagues from* St. Malo *in* Bretagne *.

The obſervations on the rain and wind, made at the caſtle of *Pontbriand*, two leagues from *S. Malo* in *Bretagne*, may be ſeen in the memoirs of the year 1704.

This is the continuation of theſe obſervations, made by M. *Pontbriand* with great exactneſs in the ſame place during the year 1705, ſent by him to M. *du Torar* of the royal academy of ſciences, in order to be communicated to the academy, and compared with the obſervations made at *Paris*, by M. *de la Hire*.

JANUARY.

Days.	*Rain-water.* Lines.	*Winds.*
8	2	North N.E.
10	2	S.S.E.
11	1½	S.S.W.
12	1½	W.N.W.
13	3½	N.N.W.
14	1½	W.N.W.
17	1½	E.N.E.
19	0½	E.N.E.

Total 14 lines.

---

* Jan. 9, 1706.

FEBRUARY.

Days.	*Rain-water.* Lines.	*Winds.*
5	2	Eaſt S.E.
10	1	N.N.E.
13	0¼	N.W.
14	2	N.N.W.
16	2½	S.S.W.
17	0½	S.S.W.
18	2½	E.S.E.
25	1½	E.N.E.
28	0¼	E.N.E.

Total 13 lines.

MARCH.

Days.	*Rain-water.* Lines.	*Winds.*
4	3½	North-Eaſt.
8	0¼	S.S.W.
10	0½	S.S.W.
13	1½	S.S.W.
14	0¼	S.S.E.
24	1½	S.E.
25	1½	S.E.
26	3¼	S.S.W.
27	5	S.S.W.
28	0½	S.S.W.
29	3½	S.E.
30	0½	N.E.
31	0½	N.E.

Total 23¼ lines.

APRIL.

Days.	Rain-water. Lines.	Winds.
1	$0\frac{1}{2}$	Weſt.
2	$1\frac{1}{2}$	N.W.
3	$0\frac{3}{4}$	N.W.
4	2	N.W.
5	$2\frac{3}{4}$	W.N.W.
7	$3\frac{3}{4}$	W.S.W.
8	$2\frac{1}{4}$	W.S.W.
9	$1\frac{1}{2}$	S.S.W.
10	$0\frac{3}{4}$	S.S.W.
11	1	N.W.
12	1	N.W.
15	1	N.W.
16	$0\frac{3}{4}$	S.S.W.
19	$1\frac{3}{4}$	N.E.
20	$0\frac{1}{4}$	S.S.E.
24	$4\frac{1}{2}$	N.E.
25	$0\frac{1}{2}$	N.W.

Total 26 lines.

MAY.

Days.	Rain-water. Lines.	Winds.
4	3	Weſt.
5	$0\frac{3}{4}$	N.W.
18	4	N.W.
26	$0\frac{3}{4}$	S.E.
28	$0\frac{1}{4}$	N.E.

Total $8\frac{3}{4}$ lines.

JUNE.

JUNE.

Days.	*Rain-water.* Lines.	*Winds.*
13	4¼	South-S.W.
16	0¼	W.S.W.
17	2	W.S.W.

Total 7½ lines.

JULY.

Days.	*Rain-water.* Lines.	*Winds.*
2	0¼	North.
3	1	N.
6	2½	N.W.
10	0½	N.W.

Total 4¼ lines.

AUGUST.

Days.	*Rain-water.* Lines.	*Winds.*
7	2¼	South-Weſt.
21	3	W.S.W.
23	1½	S.W.
24	1	N.W.
27	2½	E.S.E.
31	1	W.S.W.

Total 11¼ lines.

SEPTEMBER.

Days.	*Rain-water.*	*Winds.*
	Lines.	
3	2¼	North-Weſt.
4	2½	N.N.W.
5	2	W.S.W.
6	3½	W.S.W.
8	3	N.W.
10	1¼	S.W.
15	0½	N.W.
23	3	N.N.W.

Total 18 lines.

OCTOBER.

Days.	*Rain-water.*	*Winds.*
	Lines.	
6	5½	North-Weſt.
12	1	N.W.
13	1	N.W.
14	0½	N.W.
15	2	S.S.W.
16	0½	S.S.W.
17	3½	S.S.W.
18	6¼	S.S.W.
19	3	S.S.W.
29	2	S.S.W.
30	3	N.W.
31	3	N.W.

Total 31¼ lines.

NOVEMBER.

Days.	*Rain-water.* Lines.	*Winds.*
2	3	Eaſt.
3	2½	N.E.
4	1	N.E.
9	0½	N.E.
15	1½	E.S.E.
18	1½	N.E.
20	1	N.W.
21	0¼	S.W.
22	0½	N.W.
25	3½	N.W.
26	0¼	N.W.
28	0½	N.N.W.
29	5	S.W.
30	4½	N.W.

Total 26 lines ½.

DECEMBER.

Days.	*Rain-water.* Lines.	*Winds.*
1	1	North-Weſt.
3	5	N.W.
4	5¼	W.S.W.
5	2½	N.W.
6	1½	S.W.
10	3½	S.W.
13	2½	S.S.W.
14	1	S.S.W.
15	8½	S.S.W.
16	3	N.W.
17	2½	N.W.

Days.	*Rain-water.*	*Winds.*
	Lines.	
18	1¼	North-Weſt.
19	7	W.S.W.
20	3½	N.W.
21	2½	N.W.
22	5½	S.W.
24	3	W.S.W.
25	3	W.S.W.
26	4	W.S.W.
29	1	S.S.W.
*30	5	S.S.W.
31	5	S.S.W.

Total 75 lines.

Total quantity of the rain-water, which fell at *Pontbriand*,

	Lines.
During the year 1705.	260
And in the year 1704.	284
Difference	16

---

* On the 30th, there was a violent tempeſt, which made great confuſion in *Bretagne.*

III. *Other obſervations on the rain, which fell during the year* 1705, *at* Lyons, *communicated to M.* Caſſini *by F.* Fulchiron, *a jeſuit* *.

	Lines.		*Lines.*
Jan.	7	July	12¼
Feb.	27	Aug.	15
March	18½	Sept.	6¼
April	10½	Octob.	47¼
May	23½	Nov.	14¼
June	26½	Dec.	63½

	Lin.		*In.*	*L.*
Sum total	272½,	or	22	8½
In the year 1704 it rained			15	4½
Difference			7	4

IV. *Obſervations of the barometer and thermometer, made at different places during the year* 1705, *by M.* Maraldi †; *tranſlated by* Mr. Chambers.

The barometer in the weſtern tower of the obſervatory was at its greateſt height on the three laſt days of *February*, when we found it at 28 inches, 3 lines ½, the wind blowing at N. and N.E. its leaſt height was 26 inches, 8 lines ½, at which we found it on the 19th of *December*, the wind blowing violently at ſouth and ſouth-eaſt, with rain, the difference therefore or play of the barometer this year was 1 inch, 7 lines.

The thermometer made by M. *Amontons*, ſituate in the weſtern tower of the obſervatory, was found on the 2d and 3d of *Feb.* at 51°, 11 lines, which was its leaſt height.—— By the obſervations made at *Genoa*, by the marquis *Salvago* with a thermometer, like ours, the ſame 3d of *Feb.* was alſo the time when it was loweſt at that city, being there at 52°, 10 lin. which is almoſt a degree higher than at *Paris*.——— By the obſervations made at *Lyons*, the thermometer was alſo found at its loweſt pitch for this year on the ſame 3d of *February*.

---

* Jan. 16, 1706.

† The ſame date.

At *Paris*, the thermometer was found at its higheſt pitch for this year on the 6th of *Aug.* when it roſe to 57°, 3 lines, the wind blowing at ſouth-eaſt. On this day a thermometer of M. *Caſſini*, which had ſtood 35 years, burſt, by reaſon the liquor roſe all into the tube, where wanting room for other rarefaction, the glaſs was forced to give way.——— At *Genoa*, a thermometer of M. *Amontons* roſe on the 2d and 3d of *Auguſt* to its higheſt pitch for this year, which was 56°, 8 lines, the wind blowing at north; ſo that the thermometer did not riſe ſo high at *Genoa* this year as at *Paris*, by ½ a degree.—At *Lyons*, the thermometer roſe to its ſtate on the 8th of *Auguſt*, two days later than at *Paris*.—At *Montpellier*, M. *Bon* found one of M. *Amontons* thermometers riſe to its higheſt ſtate, which was 58°, 2 lines, on the 30th of *July*; ſo that at *Montpellier*, it was almoſt a degree higher than at *Paris*, the vehement heat of this day burnt moſt of the vines; and it was obſerved, that upon expoſing the thermometer 28′— to the naked ſun, it roſe to its higheſt pitch poſſible; that is, to 75 inches, which is the degree laid down by M. *Amontons* for the heat of boiling water.

V. *Remarks on the bivalve ſhells, and firſt concerning muſcles, by M.* Poupart *; *tranſlated by Mr.* Chambers.

Muſcles are a kind of little fiſhes, lodged between two ſhells, which are uſually concave and convex.

There are both ſea and river muſcles; the latter are divided into ſeveral ſpecies; ſome whereof we ſhall take occaſion to conſider hereafter.

They all open, ſhut, walk, and ſome of them float on the water. They all likewiſe creep half out of their ſhells, and back again. Add that all of them ſhed their milt, and breathe, or rather draw in the water with their gills, and hide themſelves in the ſand or mud alike.

How they open their ſhells.

Shell-fiſhes, in all probability, were the firſt fiſhes that mankind knew, or bethought themſelves of eating; for it was a good while ere the line, hook, net, and other utenſils of fiſhing were invented. But for the ſhell-kind, the ſea ſpontaneouſly caſts them aſhore; ſo that nothing was required of the firſt people, but to ſtoop and take them up. The manner, however, of their opening has not been yet diſcovered; tho' a very able *Dutch* anatomiſt has ſought it with great pains, as appears in his treatiſe upon the anatomy of the muſcle. This ſhews, that the ſimpleſt and leaſt intricate things are ſometimes the moſt difficult to diſcover.

All the kinds of muſcles, and even all ſhell-fiſhes with a double ſhell, have a ſort of leather ligament, which binds the two ſhells together at the poſterior and thicker part, called the *heel* thereof. And 'tis by the ſpring of this ligament, that the ſhells are opened. This ligament is the more extraordinary, as it has two ſeemingly oppoſite effects, both to keep the ſhells cloſe together, and to open them. The manner hereof is as follows.

---

* Feb. 20, 1706.

When the muſcle, or other fiſh, cloſes its ſhell, by contracting the muſcles thereof, the ligament, which is between the edges of what we call the heel, becomes compreſſed of courſe; and thus remains ſo long as the muſcles remain in a ſtate of contraction. But this ligament, tho' very hard, is yet ſomewhat ſpongeous; ſo that coming to ſwell, upon a relaxation of the muſcles, it thruſts the two ſhells aſunder, and makes them open a little.

Different kinds of fiſhes have different ligaments, that of the river muſcle is a kind of hinge faſtened externally on the edge of the two ſhells, and reaching outwards. It would not be well incloſed between the edges of the ſhells, tho' ſince in ſuch as have a ginglimus to be mentioned hereafter, it would cover and render it uſeleſs, and in ſuch as have none, the edges are too thin to contain the whole ligament.

The ligament of the ſea-muſcle differs from the river-kind, in that it is not faſtened behind the ſhells, but partly between the edges, ſo as none of it comes without, but a little of it hangs over within the ſhell, by reaſon the edges are not thick enough to hold it all. To ſupply in ſome meaſure this defect, it is ſurrounded with two ſtrings, ſtrongly faſtened on the inner edges of the ſhell, whoſe thickneſs is increaſed hereby. Theſe ſtrings are hard and porous, and ſeem ſuperadded to the ſhell, and of a different ſubſtance, 'tis probable, that the tracks graven in them are not altogether uſeleſs, tho' I have not yet diſcovered what purpoſe they may ſerve. The office of the ſtrings themſelves, is to give thickneſs to the edges of the ſhell, and hereby enable them better to compreſs the ligament between them, which the bare edges of the ſhell could not do, as being too thin, and conſequently their compreſſion too weak to occaſion a ſpring ſufficient to open the muſcle. ——— The ligament that opens the oyſter's ſhell is very different from that of the muſcle-kind; it does not enter at all within the cavity of the ſhell, as that of the ſea-muſcle does; nor does it reach without it, like that of the river-muſcle, but is incloſed in the heel between the ſhells, where there is room enough to hold it.

It is ſhaped like a creſcent, whoſe back or thickeſt part is turned towards the cavity of the ſhell, the horns or ſlendereſt parts looking outwards, and the middle ſpace filled with a fungous matter. Hence the ſhells finding moſt reſiſtance in preſſing upon the thickeſt part, its ſpring or elaſticity muſt be greateſt on that ſide the ſhells are to open on. It muſt be added, that this ligament does not reach to the tip of the heel, but leaves a little vacancy there, that the ſhells may have a liberty to open.

The matter of this ligament in oyſters is not altogether the ſame with that of muſcles, but more coriacious and dry. The ligament of the ſea and river-muſcle is very ſtiff and brittle; ſo that if left a little out of water, it breaks upon the leaſt opening or ſhutting of the muſcle. There is indeed a neceſſity for its being dry; for being always in water, it would otherwiſe be ſo ſoftened, as to looſe all its ſpring, whereas now it only ſoftens like a ſtrong piece of leather, and they eaſily bends or ſtraitens again, without breaking when the contraction or relaxation of the muſcle requires it, even the whole muſcle may be opened without breaking the ligament.

It would be curious enough to examine the ſeveral ligaments which open the various ſorts of ſhell-fiſhes, no doubt many things would be found therein very ſingular and extraordinary. This I note in favour of ſuch as take delight in unfolding the ſmalleſt myſteries of nature, whoſe curioſity ſhould always have ſomething left it to be employed upon.

The manner how muſcles ſhut.

All muſcles cloſe their ſhells by the contraction of two large fibrous muſcles faſtened withinſide each end of the ſhells, which muſcles are too well known to need further deſcription, I ſhall only add, that the ſhells ſhut ſo cloſe, that water can hardly paſs them: the manner of their ſhutting is as follows.

The ſhells of all muſcles are bordered round with a kind of membrane, which may be called their *epidermis*, as being a continuation of the exterior *ſtratum* of ſuch ſhells. Theſe membranes fit ſo cloſe to each other, that when moiſtened, the ſmalleſt drop of water cannot eſcape from the muſcle, without an expedient of this kind, it had been difficult ſo to form the edges of hard thin brittle ſhells, as to render them impervious to water.

Beſide this membrane round the inner edge of each ſhell runs a ligament, which bearing one againſt another, when the ſhells are ſhut, proves likewiſe an obſtacle to the water's eſcaping, and even hinders the edges of the ſhells from breaking by the violent contraction of the muſcles.

Some muſcle-ſhells are not only bound together by the contraction of their muſcles, and by the ſpringy ligament above-mentioned, but likewiſe by long grooves or furrows, which receive a ſort of tongues, or ſlips in their whole length. At the end of theſe grooves, immediately under the heel, is a dented pin, which enters a dented cavity of the other ſhell; on the edges of which cavity are two little dented eminences, which are likewiſe received in two little dented cavities of the other ſhell, the dentile of the epiphyſis and cavities thus mutually receiving each other like thoſe of the cranium.

But this ginglymus is not found in all the ſorts of muſcles; the ſea-muſcle, that larger kind found in ponds, which ſometimes grows a foot long, and that other which I denominate the creſted kind, from an eminence in form of a cock's comb near the heel, have no ſuch articulation.

Of the progreſſive motion of muſcles.

From the ſtructure of muſcles, one would imagine they ſhould have no other motion but what they receive from that of the water; and yet they all walk, and ſome of them float on the ſurface of the water; their manner of walking is thus:

Being laid on the flat ſide of their ſhells, they iſſue partly out of the ſame, in figure of a tongue, and make little vibrations therewith, to dig the ſand or mud of the river. In thus digging, they dip gradually on one ſide, till at length they are found on the edge of the ſhell, with their back or heels upwards. In this ſtate they gradually advance their head; that is, the tip of the tongue above-ſaid, for a minute or two, and then bear or reſt upon it, in order to draw the ſhell towards the ſame, much as water-ſnails are ſometimes ſeen to do. They repeat this motion ſo long as they are diſpoſed to walk; and thus form a ſort of irregular tracts, which are ſometimes 3 or 4 yards long, in which they lay half hid.

In the ſummer time, we find ſtore of theſe tracks in rivers where muſcles abound, and never miſs of a muſcle at the end of each tract. 'Tis in this manner theſe little fiſhes ſeek their living, walking this way and that, with the heel foremoſt, and ſtill plowing the ground with the edge of their ſhells.

Theſe hollow tracks ſerve as reſts to ſuſtain the muſcles on the edge of their ſhells, and may likewiſe probably ſerve to catch and detain the ſpawn of ſome fiſhes, or other kinds of food proper for them.

One would think it had been better, if the point of the ſhell had gone before the heel, ſince being thinner and ſharper, it would have been fitter to dig the ground, like the ſhare of a plow, which always goes with the point foremoſt.

I do not find any muſcular ſtructure, whereby the muſcle ſhould be pulled out of its ſhell, which leads me to imagine, that this is only done by glutting and dilating themſelves with water. This they will ſwallow ſo plentifully, that I have drawn half a glaſs full from the pond muſcle.

One of the moſt conſiderable effects of their walking is, that by means thereof, they are able to meet and rub or ſpawn together.

I have found no eggs in any muſcles, yet in the ſummer-time, there is both milt and ſtore of mucus in the ſame muſcle, which makes me conjecture, that they may be hermaphrodites.

The large gland of the creſt of the muſcle is always full of a very white milt in the month of *September*, in which milt there is this remarkable, that it coagulates as ſoon as thrown in water. This coagulation makes me imagine, that the muſcle does not caſt its ſpawn in the water, ſince it would hereby be rendered uſeleſs for generation. I rather ſuppoſe therefore, that the muſcle inſinuates its milt into another muſcle, at the time of propagation. There is even reaſon to ſuppoſe, that the ſame happens in other fiſhes, and that they do not caſt their milt in the water, as is vulgarly imagined.

To ſee this milt, the large gland of the creſted muſcle, which makes the firmer and better part of the fiſh, muſt be cut in two, upon which a flux thereof will be ſeen, as if the whole had been melted down, gathering this milt with a knife-blade, and preſently caſting it into the water, it inſtantly coagulates into little grumes or clots.

Of the fluttering of a kind of muſcle.

Ariſtotle tells us, that he had been informed of a large ſort of muſcle, which flutters in the water; and we have already obſerved, that the philoſopher was not deceived. For I have had the the good fortune to ſee the pond-muſcle abovementioned fluttering after this manner. — The reaſon I take to be this.

This muſcle has a very thin light ſhell, yet ſo large that it may beat the ſurface of the water, as birds do the air with their wings. On the back of the ſhell is a large ſpringy ligament, and within are two ſtrong muſcles to ſhut it. This is enough to make it flutter; all required to that end, being that theſe ſprings act readily one after the other, and ſtrike the air with ſufficient velocity and ſtrength. What favours this motion ſtill more is, that the ginglymus found in other ſhell-fiſhes which do not flutter, is not ſeen in this, which would prove an impediment.

The manner how muſcles bury themſelves in the ſand.

When the muſcles find it cold they bury themſelves in the ſand, and to that end they detach a part out of their ſhells in figure of a tongue, which they drag gently this way and that to move the ſand; till in half an hour's time they become wholly covered therewith.

How the muſcles re-enter their ſhells.

The muſcles are enabled to return in their ſhells by means of a muſcular membrane, wherewith the large gland, already obſerved to iſſue from the ſhell in figure of a tongue, is wholly inveſted. When this membrane contracts the gland, which is naturally ſoft and flexile, becomes hard and wrinkled, after handling it a little, as we find in ſnails on the like occaſion.

Of the ejaculation of its milt.

There is a deal of probability, that it is the contraction of the muſcular membrane abovementioned, that occaſions the milt to iſſue from the large gland through little pores, or canals, viſible therein when it is ſwelled with water; for upon compreſſing, we find the water dart in little threads to a good diſtance therefrom.

Of the voiding its excrements.

As to the voiding its excrements, we ſuppoſe it effected by the contraction of the circular muſcles of the inteſtine, which are very numerous, and diſpoſed in bundles. To ſee them, the inteſtine muſt be cut lengthways, the excrements taken that the ſhells are only membranes condenſed and hardened; and the ſame likewiſe holds of bones, which in ſome diſeaſes, become as ſoft as cloth.

An explanation of the figures, tranſlated by J. M.

Plate VI. *Fig.* 1. A river muſcle. of which the elaſtic ligament *, which makes the muſcle open, is faſtened externally to the heel of the ſhell.

Fig. 2. A ſea muſcle, of which the elaſtic ligament *, which makes the ſhell open, appears only internally toward its heel.

Fig. 3. An oiſter, of which the elaſtic ligament*, which makes it open, is hidden between the two ſhells of the oiſter.

Fig. 4. A muſcle, which I have called creſted, becauſe it has a proceſs * in the heel, in form of a creſt.

Fig. 5. The tracks, which the muſcles make and leave behind them, when they make their progreſſive motion in the mud or ſand of the rivers.

VI. *An extract of the obſervations made in* December, 1705, *by S.* Bianchini, *upon ſome fires ſeen on one of the* Appenine *mountains, by M.* Caſſini, *jun.* * *tranſlated by Mr.* Chambers.

In the way from *Boulogne* to *Florence* through the diſtrict of *Pietra Mala*, flames are commonly perceived on the deſcent of a mountain. S. *Bianchini*, who had frequently obſerved them at a diſtance, reſolving at length to go nearer and examine them, gives us the following account.

---

* Aug. 7, 1706.

p.386

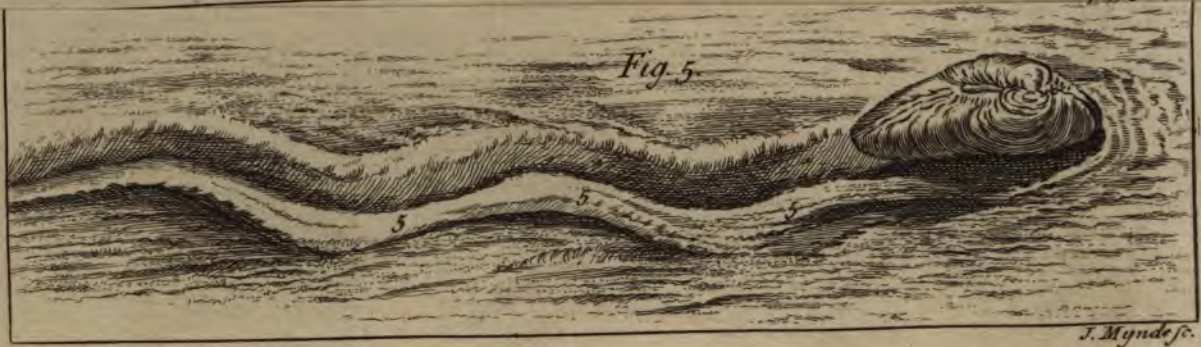


Fig. 5.

J. Mynde sc.

After ſeeing a living flame ariſe without interruption, and without any matter to ſuſtain it other than what it receives from the ſituation of the ſubterraneous places, found in the mountain of *Pietra Mala*, I make no doubt, but that the uſe of fire in our arts was 1ſt communicated and rendered durable by ſome of theſe natural mines, or ſprings of flame, obſerved in this mountain.—— About the flames, I found ſnow and ice, not above 4 feet diſtant, which continued till about noon. I had ſeveral ſtrangers with me, who had the ſame curioſity for examining, and we had a guide, who was to look to our horſes at the top of the mountain of *Pietra Mala*, from whence we aſcended on foot in a ſouthern direction, for the ſpace of about two miles, leaving the great road on the right, and deſcending the other ſide of the mountain by a narrow path, which ended in a plain, capable of being cultivated. In the middle of certain plowed grounds, we perceived a road, from whence aroſe ſeveral little flames, which ſeemed about half a foot above the ground, as if they aroſe from wood or coal. The place where theſe flames are found, is 8 *Roman* feet broad, and 16 long, and is capable of being meaſured as well as other parts of the field, it being eaſy to walk about, and even over the flame, without fear of falling into any hole or cavern, as on mount *Veſuvius*, the ſoil is here quite diſcontinuous, and differs in nothing from the reſt, excepting that the vein of fire renders it ſomewhat firmer, and gives it more of an aduſt colour. I ſay the vein of fire, as not knowing how otherwiſe to call this unknown matter, which produces ſo many different flames diſperſed here and there, in twenty ſeveral places, all within the compaſs of 130 feet ſquare. I judged it needleſs

leſs to count each of them apart, ſince one may make flames ariſe from every ſpot of the whole compaſs, either by ſtriking the ground ſlightly, with a ſtick or other body, or by throwing ſtraw, paper, or other combuſtible matter thereon.

Yet when ſuch combuſtibles were laid in a place at a diſtance from the flames, this did not hinder their taking fire, much as paper or linnen would have done, by caſting them on a red-hot iron. In fine, we obſerve that one of the flames thus produced, after it had conſumed the matter thrown on it, continued ſtill burning, being fed with ſomething elſe ſupplied by the ſoil.

We caſt branches of thorns and other ſhrubs, gethered for the purpoſe, as we came on theſe flames, and found them burn as readily, as when thrown on a common fire, and obſerving ſome lumps of ſnow, not yet melted, within two feet of the flame, and finding pieces of ice under the ſnow at 4 feet diſtance from the flame, it not only put me in mind of what the poet ſaid, ſpeaking of mount *Ætna*, with its ſnows and fires; *Scit nivibus ſervare fidem*; but I had a mind to make the experiment of caſting ſnow and ice into the flame, I did it accordingly, and found them diſſolve in a moment, as if they had been caſt upon a common fire. The flame however was not extinguiſhed thereby; but, on the contrary, appeared more vivid, and ſpread more forcibly around.

While we made theſe experiments, all about the place we perceived a very agreeable odor, which ſeemed to iſſue from this burning ſoil, and was much like what ariſes upon burning ſome odoriferous wood, as calambouc. This odor was moſt ſenſible, when we ſtood oppoſite to the ſun, or at ſome little breeze in her face, which increaſed the flame. I gathered ſome pieces of ſtones near the flame, and a handful of duſt of the ground, which being rubbed againſt each other, produced flame, and yielded the like odor as that above-mentioned. Theſe ſtones were ſo hot at firſt, that one could hardly bear them in the hand, and they preſerved their heat for $\frac{1}{4}$ of an hour, and their ſmell much longer.

Having made theſe experiments, which appeared ſufficient to ſatisfy our curioſity as to the firſt communication of fire, and may afford ſcope enough for philoſophiſing upon ſo extraordinary an effect, we reſumed our journey towards *Fiorenzola.*

Reflections on the foregoing obſervations.

This fire, obſerved by S. *Bianchini*, bears a near relation to that obſerved in *Dauphini*, by M. *Dieulamant*, whereof an account is given in the hiſtory for the year 1669.

There is no viſible fuel of either of the flames, though this latter ſmells ſtrong of ſulphur, beſides that it burns moſt in winter and moiſt weather. The two fires have this in common, that they are on the deſcents of mountains, and ſeem both to ſpring from the ground, without any apparent cleft, whereby to communicate with any cavern underneath. They are both of them likewiſe increaſed by moiſture and cold weather, as may appear of the former by the ſnow caſt thereon, which in thawing augmented the fire.

The chief difference lies in the ſmell, which in the one is ſulphurous, and as it were aromatic in the other.

VII. *A diſſertation on the barometers and thermometers, by M.* de la Hire, *jun.* * *tranſlated by Mr.* Chambers.

We are much obliged to the philoſophers of the laſt age, for having found a method of determining the ſeveral changes which befel the air, conſidered as a heavy and an elaſtic body: a diſcovery of the utmoſt importance, as it lets us into the reaſon of a multitude of phænomena, which had infinitely perplexed all former naturaliſts, and given riſe to I know not what averſion in nature to a vacuum.

The celebrated *Galileo*, mathematician to the grand duke of *Tuſcany*, was the firſt that obſerved, that water could only riſe in a ſucking pump to the height of 32 feet; and that if the barrel of the pump exceeded this height, ſuch exceſs would remain empty. The conſequence he drew from this obſervation was, that nature had only an averſion for a vacuum at this height, which was a concluſion of a piece with the former philoſophy.

Toricelli his diſciple and ſucceſſor made another experiment in the year 1643, taking a glaſs tube 4 feet long, and open only at one end, he filled it with mercury, and thus inverted it into another veſſel full of mercury, the open end being downwards; the effect was, that the mercury in the tube ſunk ſome 20 inches, and left a ſpace in the top, which muſt neceſſarily be a vacuum.

This experiment was communicated by a letter from *Italy* to father *Merſenne*, a minim of *Paris*, who ſpread it thro' all *France*, till at length M. *Petit*, intendant of the fortifications, having imparted

---

* Nov. 13, 1706.

parted it to M. *Paſcal*, they repeated the experiment together at *Rouen* in 1646, and found it perfectly agreeable to what had been notified from *Italy*; on which occaſion M. *Paſcal* made ſeveral other experiments, whereof he formed a book, which was publiſhed in 1647. The ſame year he received intelligences that *Toricelli* had ſuſpected, that the weight of the air was the cauſe of the ſuſpenſion of the mercury in the experiment above-mentioned, which led him to make ſome further experiments, by which he was confirmed in the ſentiment of *Toricelli*, upon which he ventured to advance, that all thoſe effects which had been attributed to nature's abhorrence of a vacuum, were only owing to the weight of the air, of which he made abundant proof in a treatiſe on that head —— Such were the epocha, and order of the ſeveral experiments, whereby this noble property of the gravity of the air were diſcovered: we proceed now to a deſcription of the machines, contrived to diſcover its elaſtic power, beginning with the oldeſt, and proceeding according to the order of time.

Sanctorius, a celebrated phyſician of *Capo d'Iſtria*, bethought himſelf of a machine for diſcovering the different degrees of heat in feveriſh perſons, which he called a thermometer, not conſidering, as we have all the reaſon to imagine, that the ſame machine might ſerve to ſhew the changes which befal the air as its bulk is increaſed by different degrees of heat; and that this would be of more advantage to the publick, than all its application to the buſineſs of medicine.

This thermometer conſiſted of two glaſs balls, faſtened to a glaſs tube, bent at the bottom cloſe to the lower ball; the upper ball, which had no communication with the external air, and all the upper part of the tube were full of common air, and the reſt with part of the lower ball, which was open a top, were filled with *aqua ſecunda.*—— From this conſtruction, it eaſily appears, that when the air of the upper ball was dilated further by a greater degree of heat, it would compreſs the *aqua ſecunda* in the tube, and oblige it to deſcend; and on the contrary, would let it riſe again, when the ſame air came to be condenſed into a leſs compaſs.

This inſtrument, tho' ſubject to great irregularities, was allowed very curious by all the learned, and put in uſe accordingly, till ſuch time as the barometer was diſcovered, upon which a great defect was perceived in it, *viz.* That it acted likewiſe as a barometer, which might frequently deſtroy all its effect as a thermometer, for that the air in the lower ball, communicating with the external air, would act likewiſe on the liquor, and oblige it to riſe or fall, according as it ſhould be more or leſs heavy.

This diſcovery of the barometer proved a great blow to that of the thermometer; but it laſted not long. Some learned men at *Florence*, having contrived to make another, without the defect of the former. I have not been able to diſcover any date of the time when this latter was invented, except in *Otto Guerick*'s book, intitled, *Experimenta Magdeburgica*, printed in 1672, wherein he mentions its being diſcovered about 30 years ago; and in M. *Petit*'s *Diſſertations Academiques*, printed in 1671, wherein the invention is attributed to the academy *Del Cimento*, who firſt gave the deſcription and figure thereof in their *Saggi di naturali eſperienze.*

This which ſhould be called the *Florentine* thermometer, and which is that now moſt in uſe, being found very commodious for all kinds of experiments, as well as eaſily portable, is very ſimple in its make, conſiſting only of a glaſs ball, to which is faſtened a tube hermetically ſealed a top, whoſe diameter and length are ſo proportioned to the diameter of the ball, which, with a part of the tube, is to be filled with ſpirit of wine, as that the ſpirit, when dilated by the utmoſt degree of heat of our climate, may not quite fill the tube, nor when condenſed by the utmoſt degree of cold, ſink wholly into the ball.

But tho' this thermometer have great advantage, it had one conſiderable defect, which was, that we could not by means hereof compare the temperature of the air of one country with that of another, unleſs either one and the ſame thermometer were carried into both places, or ſeveral thermometers were divided to the ſame degrees of heat; but M. *Amontons* found a remedy for this, and made the thermometer truly univerſal, without changing any thing in its ſtructure, by fixing a degree of heat, *viz.* that of boiling water, which is the ſame every where, whereby all the other degrees are to be directed, ſo that nothing further ſeemed deſirable on this head; yet we find this year a further attempt of M. *Nuguet*, who has publiſhed a treatiſe, intitled *A New Diſcovery of a thermometer*, long ſought for by the royal academy, free from the defects of other thermometers, and containing all the advantages found ſeparately in all others hitherto invented.

Not to mention, that he had no grounds for quoting the academy, as engaged in ſuch a reſearch, we obſerve, that his thermometer is much like M. *Huygens*'s barometer. It conſiſts of a glaſs ball hermetically ſealed, and full of air, condenſed by the cold of iced water, and of 4 cylindrical tubes, cemented to each other, which all together make only one ſingle bent tube, the bend being downwards. This tube is filled like the double barometer, tho' with ſome precautions to be mentioned hereafter, by which means the ſpace from the top of this tube to the middle of the firſt tube, is emptied of groſs air; the reſt of the ſpace, as far as the middle of the third tube, which is over the bend in the other branch, being full of mercury. Laſtly, over the mercury there is ſpirit of wine, as far as the middle of the fourth tube, to whoſe top the ball is faſtened, which is full of air as well as the reſt of this tube.—— Now, from this ſtructure it eaſily follows, that as the heat increaſes, the ſpirit of wine muſt deſcend, and riſe again as the cold returns, by reaſon the air in the ball, and part of the tube being dilated by the large heat, will oblige the ſpirit of wine to deſcend, and when condenſed again by the cold, will leave the ſpirit at liberty to re-aſcend; but neither this ſtructure, nor manner of filling, I doubt, will appear ſo ſimple as that of the *Florentine* thermometer. Let us now ſee, how he fixes the ratio of his tubes, upon which the whole ſtructure of his thermometer depends.

The proportion which he has taken between the tube, wherein the ſpirit of wine moves, and the other tubes, wherein the mercury is contained, and between the weight of ſpirit of wine and that of mercury is ſuch, that when the liquor is arrived at the top of the third tube, which denotes the extreme heat of ſummer, the air in the ball will ſupport 4 inches of mercury more than it will ſupport when the ſame liquor arrives at the entrance of the ball, which denotes the greateſt degree of cold. The reaſon he gives for this proportion is, that the incloſed air will, by the higheſt degree of the ſummer's heat, be able to ſuſtain 4 inches of mercury more than it ſuſtains in the greateſt winter's cold.

This paſſage, which is taken from his own writing, will afford room for ſeveral obſervations. ———— 1ſt, That he ſays nothing of the diameter of the ball, wherein the air is incloſed, which it ought by all means to be conſidered, ſince by ſome experiments, which we have made, it appears, that different bulks of incloſed air, when expoſed to the ſame degree of heat, will ſuſtain the mercury at different heights; whence will ariſe a neceſſity of making thoſe balls perfectly equal in all his thermometers, and the tubes equal likewiſe, or in the ſame proportion, which is almoſt impoſſible in the execution.

2dly, That he does not mention in what part of the earth the difference between the utmoſt ſummer's heat, and the winter's cold, will ſuſtain 4 inches of mercury; and if, as we ſuppoſe, he means it at *Paris*, where the terms of heat and cold have lately been ſettled, yet to have any of theſe thermometers of uſe in other countries, they muſt be made there on purpoſe; thoſe made at *Paris* being uſeleſs, by reaſon the greateſt degrees of heat and cold, whereon the whole ſtructure is found, change according to the country, which will induce a neceſſity of finding and fixing them, which is no ſmall difficulty.

3dly, He ſhould have expreſſed whether this air, which is able to ſuſtain 4 inches of mercury more in ſummer than in winter, be incloſed in a ſtate of condenſation, ſince, from his account, it does not appear, that this air is condenſed; and

Yet that in the ball of his thermometer is condenſed by the cold of freezing water, which leads us into a great difficulty, by reaſon the air whereon his thermometer is built ſeems of one kind, and that in the ball of another, and yet he ſeems to infer the effect which the latter will produce from that produced by the former.

4thly, That ice or freezing water will always be neceſſary for the making of his thermometers.

5thly, That regard muſt be had in making his thermometers to the different heights of the atmoſphere, which occaſion alterations in the body of air.

6thly, That notice muſt likewiſe be taken of the degree of dryneſs or moiſture of the air, which may produce ſome alterations in his thermometer.

We proceed now to examine the precautions wherewith this thermometer is to be filled.—— Ere the extremity of the ball be ſealed, care muſt be taken that the ſpirit of wine contained in the tube, which is joined to the ball, correſpond by its upper part to the degree of the ſcale of the common thermometer, which exactly expreſſes the degree of cold of freezing water, wherein they are plunged; and ſince he proportions the quantities of water and ice in ſuch a manner, as that the cold ariſing from the mixture of the two, may ſuffice to make the liquor in the common thermometer ſink to the 34th degree of its ſcale, he introduces the ſpirit into this tube, till ſuch time as its upper extremity correſpond to a point, which expreſſes the 33d degree of the ſcale of his themometer.

'Tis evident, that in this method of filling his thermometer, he will always need one of the *Florentine*

rentine thermometers, and that it can never be univerſal, ſince only thoſe made by the ſame common thermometer will be capable of being compared together, for ſuppoſing them equal in every thing elſe, the 33d degree of all the common ones will never expreſs the ſame degree of cold, as this degree is not determined by any fixed uniform cauſe.

Thus much for the general difficulty in the ſtructure of M. *Nuguet*'s thermometer: I ſhall now proceed to a compariſon of its effects, with thoſe of a very good *Florentine* thermometer, which has been uſed by my father and ſelf upwards of 30 years. ——— On the 25th of *June*, at a quarter paſt 2 in the afternoon, the ſky being very clear, I expoſed the *Florentine* thermometer to the ſun, in a place perfectly free from wind, and that of M. *Nuguet* aſide of it. This latter being placed very perpendicularly, I found it ſink to 93°—½, and a few minutes after riſe again to 89°—½, where it fixed, tho' ſtill expoſed to the ſun, by which it appears, that this aſcent could neither be owing to any cooling of the air during the experiment, ſince, in that caſe, it muſt have continued cooling, and conſequently the ſpirit of wine have continued riſing, unleſs we ſuppoſe the air to have only been cooled for a few minutes, in which caſe the ſun's rays would have heated it again, and conſequently the ſpirit muſt have ſunk again, nor to a dimunition of the effect of the ſun's ray, by reaſon of the difference of his height above the horizon; ſince, after ſinking to its loweſt in a ſhort time, when it began to riſe again, it ſhould have continued riſing to the end of the experiment. The other thermometer roſe to 86; after which there was no ſenſible riſe, tho' they both ſtood in the ſun 25′—. After which, I removed them into a room, which was open to the eaſt, and into which the ſun could not play, where when they had ſtood long enough to ſettle, I found that of M. *Nuguet* riſen to 78°—½, and the other ſunk but to 64°—½; the difference therefore between that of M. *Nuguet*, when expoſed to the ſun, and in the room, was 11°, which are equivalent to 3 inches, 3 lines ½; and the difference between the *Florentine* one in thoſe two ſtates, was 21°—½, which are equivalent to 7 inches, 3 lines ½; conſequently the latter was more ſenſible by half than the former.

It remains to be explained, why, upon expoſing the new thermometer to the ſun, it firſt ſunk, and then roſe again. The reaſon whereof is this, that the heat acting on the air and the ſpirit of wine at the ſame time, and the air being more ſuſceptible of dilatation, it made the ſpirit of wine ſink very readily at firſt, which is the only advantage that I can perceive in this new thermometer above others; but the ſpirit of wine at length warming and dilating it, compreſſed the air, and by means hereof gained 4′———— in height, which is a proof, that the new thermometer ought to be conſidered, as compoſed of two others: one an air thermometer, as that of *Sanctorius*; and the other a ſpirit thermometer, as that of the *Florentine* academy, but which act againſt each other.

It may not be here amiſs to advertiſe thoſe unacquainted with the laws of dioptricks, that they are not to attribute the great riſe of the *Florentine* thermometers, when expoſed to the ſun, to the ſpherical figure of their phials, which can have no effect thereon, more than if the ſpirit were expoſed naked in any other veſſel; for if by the curvity of the phial, the rays which fall on it be collected as they paſs through the ſpirit, and thus heat that part they touch more ſtrongly than they would otherwiſe do, they will deſert another part of this ſpirit, and have no effect at all thereon; ſo that one compenſates for the other.

VIII. *Reflections on the obſervations ſent to M.* le Comte de Pontchartrain, *by F.* Laval *Regius profeſſor of hydrography*; *by M.* Caſſini *.

The ſituation of the obſervatory at *Marſeilles*, in view of the horizon of the ſea, gives F. *Laval* the convenience of obſerving the horizontal variations, which are commonly aſcribed to the refractions of the viſual rays.

Thoſe which he has hitherto made, being corrected by the rule which we have given on occaſion of the obſervations made on the mountain of *Notre Dame de la Garde de Toulon*, ſhew, that the horizon of the ſea is ſeven ſmall leagues diſtant from the obſervatory; and that the obſervatory is elevated 175 feet above the ſurface of the ſea.

It is a thing that would merit being examined by the levelling made from the obſervatory to the water of the ſea.

We have obſerved, that the apparent horizon of the ſea is often ſeen lower than the true horizon, which is not always diſtinguiſhed. For there is about the true horizon an edging of the ſurface of the ſea, which is not diſtinguiſhed from the ſky. We judge of it by the ſummit of ſome iſlands, which are diſcovered beyond the horizon, and ſeem ſometimes to be elevated into the ſky above the apparent horizon of the ſea, above which they make a reflection, which forms a reverſed image of the mountain, contiguous to that of the mountain ſeen directly. Theſe two images form a total one, divided into two equal and ſimilar parts, by the true horizontal line, which is diſtinguiſhed only by this appearance.

---

* March 13, 1706.

The variation of the apparent depreſſion of the horizon hitherto obſerved by F. *Laval*, amounts to no more than a minute and half, the ſmalleſt having been 13′ ½, and the greateſt 15′; but what is moſt obſervable is, that the greateſt depreſſion was obſerved when the ſea was higheſt. The high ſea ought rather to contribute to make the horizon of the ſea appear higher; it is true, that at ſo great a diſtance the elevation of the ſea by the tempeſt would not be ſenſible; and thus the difference of the apparent height of the horizon ought to be aſcribed rather to the different temperaments of the air, which cauſe refractions in them.

We have ſometimes ſeen the ſun begin to appear in the morning above the horizon, a little elevated above the ſurface of the ſea, of an oval figure, with its length according to the horizontal line: it grew larger by degrees upwards and downwards, and was divided into two by the horizontal line, till the upper part was detached from the lower, each part growing round, and forming at laſt, as it were, two ſuns; one of which was detached from the other, and receded from it, upon the common line, perpendicular to the horizon, till the lower one, which we ſuppoſe to be cauſed by the reflection on the ſea, ceaſed to appear. F. *Laval* once obſerved the ſun to ſet in form of a bonnet, detached from the whole apparent horizon: this appearance might have been cauſed almoſt in the ſame manner, as we have explained thoſe we have obſerved formerly.

The greateſt declination of the ſun, which F. *Laval* has found in ſummer, by the obſervations of the meridian heights of the ſun, corrected by our elements of the refractions and parallaxes, comes very near that which we had determined, by the obſervations made 50 years ago, to be 23° 29. By the winter obſervations, F. *Laval* found it ſmaller by ½ a minute, which may be aſcribed to ſome little difference of refraction, in the meridian height of the ſun, which might be a little greater or leſs regular in winter. Theſe are very uſeful obſervations, which ought never to be neglected. This little difference may come alſo partly from the diviſions of the inſtruments, which are not ſo fine, but that we may doubt of ſome ſeconds in a great many degrees. In an inſtrument of 4 feet *radius*, 5 ſeconds occupy no more than $\frac{1}{48}$ part of a line, which is not very ſenſible. In the greateſt inſtruments, we perceive a little trembling in the ſun and other ſtars, which may be aſcribed to the different degrees of the temperature of the air through which their rays paſs; which leaves ſome little doubts in the obſervations. The obſervations near the horizon are ſubject to variations, by the different temperatures of the air, as appears by the uſe of teleſcopes, which in great heats ſhew an appearance of boiling in diſtant objects.

The objects ſeen together by a horizontal ray, are not always ſeen alike. We ſee from the ſame window, and from the ſame point, a mill at a diſtance, partly hidden behind a neighbouring building, ſomtimes quite elevated above the building, and ſometimes, as it were, immerged below it, and entirely hidden. Thus the proof that we make of inſtruments, by the direction of their *axes* to a diſtant object, is not always exempt from ſome little error ; a viſual ray, which paſſes thro' the ſame *medium*, not being always ſtrait, or always crooked in the ſame manner, its curvature varying according to the different temperatures of the interpoſed air.

As for the obſervations on the *eclipſes* of the firſt ſatellite of *Jupiter*, which F. *Laval* has continued to make as often as the weather permitted him, he admires their conformity to the calculations, inſerted in the book of the *Connoiſſance de temps*, which they have taken care to compoſe, by uſing the corrections which I gave 8 years ago, which conſiſt in ſubſtracting four minutes of time from the epoch, in ſubſtracting alſo one ſecond from the 25 revolutions of the firſt ſatellite, and in augmenting the firſt inequality by $\frac{1}{30}$ part. Theſe corrections very often reduce their calculations to the ſame minute that the obſervations give, which is a great confirmation of the elements upon which the calculations are founded. Theſe elements depend not only upon the proper motion of the ſatellite, but alſo on the motion of *Jupiter* and his inequalities, on the motion of the ſun, on the ſituation of the nodes of the ſatellite, and of thoſe of *Jupiter*, on the mutual inclination of their orbits ; and laſtly, on the *hypotheſis* uſed to determine the equation of time.

They are always numerous enough to leave room to queſtion, whether any imperceptible errors, to which each of them is expoſed, as all the elements of aſtronomy are, might not make ſome error conſiderable enough to require new corrections in a little time. We are always attentive to them, to endeavour to bring the theory nearer and nearer to perfection; but hitherto we may be contented with the ſtate in which it is, there being no other planets ſo well regulated, that we can determine the *phænomena* long before, in a time ſo exact, as the eclipſes of this firſt ſatellite of *Jupiter* in his ſhadow.

We are not yet ſo well ſatisfied with the *hypotheſes* of the other ſatellites, their ſecond inequality is very different from that of the firſt; ſo that we cannot aſcribe them by any means to the ſame cauſe, to which it ſeems at firſt, that the ſecond inequality of the firſt ſatellite might be aſcribed, which is that the light of the ſun, which goes to the ſatellite, and is reflected to the earth, takes up conſiderably more time in making this way, when *Jupiter* and his ſatellites are much farther from the earth towards the conjunctions with the ſun, than when they are nearer, as in the oppoſitions.

Having compared the obſervations of the eclipſes of the firſt ſatellite of *Jupiter* made at *Marſeilles*, with thoſe which we made at the ſame time at *Paris*, the difference of the meridians, which reſults therefrom is 12′ 10″; having choſen the mean between the differences which are found, which is within about a ſecond, the ſame that was found in the preceding years.

F. *Laval* has made a great number of obſervations on *Venus*, in her paſſage over the meridian, whence he has calculated her right aſcenſion and declination, which are found conformable to thoſe which we have determined by the obſervations made at the ſame time at *Paris*. He has made alſo ſome obſervations on mercury at the meridian, after which we communicated to him thoſe which we have made. We had favourable weather to obſerve *Venus* in the meridian on the day of her conjuction with the ſun.

F. *Laval* has alſo obſerved the ſpots of the ſun, which appeared during the year 1705, and he has determined the ſituation of them in the disk of the ſun according to the method which we have practiſed in concert.

An Explanation of the Terms of Art *uſed in this Volume, which were not explained at the end of the firſt Volume.*

A

A*Nimated needle*, one that has been touched by a loadſtone.

Aqua ſecunda, the *aqua fortis*, which has been uſed in the diſſolution of metals, and has thereby loſt part of its ſtrength.

Arch-ſtone, or *vouſſoir*, a ſtone cut in the ſhape of a wedge, to form the arch of a vault.

B

Bivalve-ſhells, ſuch as open with two leaves, like folding-doors.

C

Cerebellum, the hinder part of the brain. It is of a firmer ſubſtance than the *cerebrum*, or the fore part of the brain, and of a yellow colour.

Cerebrum the brain. It is uſed in contradiſtinction from the *cerebellum*, or hinder part of the brain.

D

Denier, a ſmall piece of copper money uſed in *France*.

Dura mater, a ſtrong thick membrane, which lines the inſide of the ſkull, and covers the whole brain.

E

Equilibrium, an equal weight of two bodies, when compared with each other. When two bodies are thus balanced, they are ſaid to be *in equilibrio.*

Extrados, the outſide of a vault.

I

Impoſt, the ſtone whereon the firſt arch-ſtone is laid.

Intrados the inſide of a vault.

K

Key of a vault, the ſtone in the middle, which is perpendicular to the horizon.

M

Medulla oblongata, the medullary part of the *cerebrum* and *cerebellum* united.

O

Oeſophagus, the gullet.

P

Pineal gland, a ſmall gland in the third ventricle of the brain, ſo called from its reſemblance to a pine-apple. *Deſcartes* ſuppoſed it to be the ſeat of the ſoul.

Pinion, a ſmaller wheel, which plays in the teeth of a longer. The notches of a pinion are called leaves

Pitta, *pita*, or *ſilk-graſs*, a ſort of *ananas*, or pine-apple, which comes from *America*. They ſtrip the leaves of their ſkin, and the inſide is a bunch of fine ſilky fibres, of which the ſavages make their fiſhing lines and bow-ſtrings, and the *Spaniards* fine ſtockings, and other curious works.

V

Vouſſoir, ſee *arch-ſtone*.

A GENERAL INDEX OF THE CONTENTS OF THE SECOND VOLUME.

A

pag.

AIR, *in winter its degree of heat when water freezes* 30
——— *its different degrees of heat* ibid.
——— *in ſpring and autumn, its degrees of heat* ——— ibid.
——— *in ſummer* ——— ibid.
——— *in the middle of* July ——— ibid.
——— *its condenſation conſidered* 251
——— *its condenſation and dilatation* 286
——— *reflections on the rules* ——— 340
——— *experiments on its rarefaction* 287
Alabaſter, *a valuable ſort of it* 7
Amber *yellow* ——— 7,225
Anjou, *an unuſual ſmell in the wines there* 219
——— *ſome extraneous foſſils found there* ibid.
Ants, *unjuſtly thought injurious to plants* 6

INDEX.

pag.
Apennine *mountains, ſome fires ſeen upon one of them* —— ——— 386
Aquila *overthrown by an earthquake* 137
Atmoſphere *the height of it calculated* 90

B

Barometer *obſerved* 45, 148, 234, 304, 365, 374
——— *a diſcourſe on them* ——— 173
——— *remarks on them, and on the light made by one of them, which was agitated vertically* 307
——— *a diſſertation on them* 390
——— *the height of the quickſilver conſidered* 311, 314, 316, 333
——— *ſimple and double conſidered* 151, 175
——— *without quickſilver for the uſe of the ſea* 245
——— *an irregularity of ſome of them* 347
Barometrical *experiments made on ſeveral mountains in* France —— 85
St. Bartholomew's *mountain, the height* 94
Baths, *their different degrees of heat* 80
Bee-hives *found in ſtones in* Egypt 220
Bells *uſed in diſperſing a ſtorm* ——— 9
Bezoar *oriental and occidental* —— 20
Bird *brooding on its eggs, its degree of heat* 80
Boiling-water *does not increaſe in heat* 16
Brain *petrified* ——— — 94
Bugarach, *barometrical obſervations made upon it* 87
——— *the height of it* ——— 94, 257
Burning mirror, *a proper veſſel to hold bodies expoſed to it* ——— —— 2[illegible]8
——— *the effect of it in exceſſive hot weather* 2[illegible]4

C

C

pag.

Canigou, *the height of it* 92, 94
Cantal, *the height of it* 94
Capillary tubes, *experiments on them* 319
Caſtor *the anatomy of that animal* 181
—— *its extraordinary œconomy* 197
—— *the ways of taking them* 200
Cayenne *its latitude* —— 109
Charcoal *made uſe of to hold bodies expoſed to a burning mirror* —— —— 218
Chorobates, *an inſtrument uſed by the ancients in levelling, deſcribed* —— 201
Ciſterns, *the beſt manner of conſtructing them* 59
Clocks, *reflections on the inequalities of their motion* —— —— 102
Cochineal *obſerved* —— 149
Colioure, *barometrical obſervations made upon it* 86
Condenſation *of the air conſidered* 251, 340
—— *of fluids conſidered* —— 265
Coſta, *barometrical obſervations made upon it* 87
—— *the height of it* —— 94
Courſe *of bodies tied by cords, and drawn on a horizontal plane* —— 38
Cryſtalline *humour of a ſerpent's eye* 354
Cuckow-ſpittle —— 293
Cyclops puppy —— 21

D

Deafneſs *and* dumbneſs, *a ſurpriſing recovery from them* —— 8
Declination *of the needle obſerved* 46, 149, 263, 239, 269, 273, 349, 366

INDEX.

pag.
Degrees *of heat examined* —— 80
Diſeaſes *of muſcles* —— 385

E

Earthquakes, *how cauſed by heat* 64
—— *in* Italy —— 137
Egg *within an egg* —— 358
Egypt, *a ſort of bee-hive found in ſtones there* 220

F

Fire-arms, *their recoil conſidered* 25
Fires *ſeen upon one of the* Apennine *mountains* 386
Fiſhes *petrified* — —— — 13
Flea-graſhopper —— — 294
Fluids, *the condenſation of them conſidered* 265
Force *of machines in general* —— 27
Formica-leo, *the hiſtory of it* —— 161
Formica-vulpes, *the hiſtory of it* —— 353
Foſſils *extraneous, found at* Anjou 219
—— —— *in* Germany 356
France, *barometrical experiments made upon ſeveral mountains of it* —— 85
Frictions *conſidered* —— 33

G

Germany, *an extraordinary ſtone brought from thence* —— —— 352
—— *ſome extraneous foſſils found there* 356
Glaſs, *the degree of heat which puts it in fuſion* 83
—— *why it breaks before the fire, if filled with water or quickſilver, but not with ſpirit of wine,* —— —— —— 350

pag.
Goree, *its latitude* 109
Gunpowder, *the degree of heat which makes it take fire* 83

H

Hail *of an extraordinary bigneſs* 9
Heart, *of the land-tortoiſe* 124
—— *ſea-tortoiſe* 119
Heat, *the great power of it* 46
—— *the difference between the greateſt heat and cold at* Paris 49
—— *how it may be the ſole cauſe of earthquakes* 64
—— *degrees of it related in the philoſophical tranſactions examined* 72
—— *exceſſive, felt at* Montpellier 223
—— *the quantity of it reflected by the moon* 343
Horizon *of the ſea obſerved* 399
Human body, *the greateſt degree of heat that the thermometer can receive from it* 80

I

Inacceſſible mountain, *the former account of it found to be falſe* 12
Inſect-fiſh 355
Inſects *of orange-trees* 149
Iron, *its different degrees of heat* 81
—— *converted into load-ſtone* 280
Italy, *the earthquakes there* 137

L

Lamb, *a monſtrous one* 17
Lead, *the degree of heat which puts it in fuſion* 83

pag.

Level *of a new conſtruction deſcribed* —— 200

Lights *ſeen in the heavens* ——— — 218

Lions, *the cauſe of their heat* ——— 140

Lizard *from the* Eaſt-Indies —— 21

Load-ſtone, *new obſervations on it* 273

——— *inequalities in its variation* — 277

Longitude *at ſea how it may be determined* 242, 272

M

Machines, *the force of them* ——— — 27

Magnetical *experiment* ——— ——— 10

Map *of the* Roman *empire* ——— —— 229

Marſeilles, *yellow amber found there* 7

——— *a valuable ſort of alabaſter found there* ibid.

——— *its longitude from* Paris 403

Meſſane, *barometrical obſervations made upon it* 87

——— *the height of it* ——— —— 94

Monſters, *a remarkable lamb* ——— 17

——— *a cyclops puppy* ——— —— 21

Mont d'or *the height of it* ——— 94, 305, 340

Montpellier, *exceſſive heat felt there* ——— 223

Mont Ventoux, *the height of it* ——— 94

Moon, *the quantity of heat reflected by it* 343

Motion *progreſſive of muſcles* ——— — 380

Mouſſet *the height of it* ——— 94

Mountains *of* France, *barometrical experiments made upon them* ——— —— 85

Muſcles, *remarks on them* — —— 376

Musk, *the ſmell of it produced by an accidental mixture of drugs* — ——— 352

Muſket-balls *the refraction of them in water* 296

INDEX.

pag.

N

Needle magnetical, *its declination obſerved* 46, 149, 236, 239, 269, 366
——— *new obſervations on it* 273, 349

O

Orange-trees, *inſects obſerved upon them* 149

P

Pendulum clocks, *remarks on the inequalities of their motions* ——— 102
Petrified *bodies incloſed in ſtone* ——— 13
Phœnicia, *petrified bodies found there* 14
Pipes *of lead, a better method of caſting them* 361
Plants *of the ſame ſpecies always turn the ſame way* ——— 4
Pot, *why the bottom cools when the water boils* 15
Progreſſive *motion of muſcles* ——— 380
Puceron, *a little inſect injurious to plants* 5
Puppy *with one eye* ——— 21
Puy de Violent, *the height of it* ——— 94
Puy Domme, *barometrical obſervations made upon it* ——— 88
——— *the height of it* ——— 94, 259

R

pag.

R

Rain, *the quantity of it obſerved* 43, 52, 145, 232, 236, 362, 367, 374
—— *how deep it penetrates into the ground* 55
Refraction *of muſket balls in water* 296
Reſpiration *of muſcles* —— —— 384
Roman *empire, a map of it* —— ——229

S

Sea, *a fiction of an uncommon ſhining of it* 13
—— *its horizon obſerved* —— — 399
Secretion *in animal bodies explained* 331
Seine, *the different heights of it at different times* 216
Serpent's tongues *petrified, what they are* 219
—— eye, *the cryſtalline humour of it* 354
Shark's teeth *found at* Anjou — 219
Shell-fiſhes, *remarks on them* —— 376
Ships, *a better method of ſheathing them* 361
Snails, *their ſhells generally turn the ſame way* 3
Solidity, *the natural ſtate of all bodies* 49
Springs, *the origin of them* —— —— 51
—— *ebbing and flowing* —— —— 59
Star-ſtone *moves in diſtilled vinegar* —— 12
Stomach *of a dog found in the* thorax —— 358
Stones, *the effect of vinegar on them* —— 11
—— *an extraordinary ſort brought from* Germany —— —— —— —— 352

INDEX.

pag.

T

Thermometer *obſerved* 44, 147, 235, 364, 374
——— *reduced to a fixt and certain meaſure* ——— 46
——— *a ſingular property in it diſcovered* 139
——— *a diſſertation on them* ——— 390
Tin, *the degree of heat, which puts it in fuſion* 82
Tortoiſe *of the land, its heart deſcribed* 124
——— *of the ſea, its heart deſcribed* 119
Towing *of boats improved* ——— 360
Turning *of bodies the ſame way* ——— 3

V

Valves *of pumps* ——— 22
Variation *of the loadſtone, inequalities in it* 277
Vault *the figure of its* extrados ——— 140
Vernal *froths* ——— 293
Veroneſe, *petrified bodies found there* ——— 14
Veſſel, *how to be raiſed upon the hold* ——— 116
Vinegar, *the effect of it on ſome ſtones* 11

W

Water *kept for many years* ——— 7
——— *the quantity evaporated through the leaves of plants* ——— 54
——— *how preſerved in long voyages* ——— 222
——— *its reſiſtance, and the refraction of musket-balls therein* ——— 296
——— *its aſcent in capillary tubes* ——— 319

Weather *obſerved* — — — 233, 363
Winds *obſerved* — — 233, 236
Wines *of* Anjou, *an unuſual ſmell in them* 219
Worms *are hermaphrodites* — — 16
——— *their manner of copulation* — 17
——— *how to preſerve inlaid works from them* 223
——— *with two heads* — — 354

AN
INDEX
OF
AUTHORS NAMES.

A

M. Amontons 22, 23, 24, 27, 30, 32, 33, 34, 35, 36, 45, 46, 64, 72, 139, 173, 216, 217, 223, 245, 265, 287, 311, 314, 316, 333, 343, 349, 366, 374, 375, 393
M. Antoine, 17, 19, 20
Ariſtotle, 382

B

Bartholin, 98, 100, 229
M. Bernoulli, 328
S. Bianchi, 13
S. Bianchini, 386, 389
M. Bielke, 98, 100
M. Blondel, 70
M. Bon, 375
M. de Bonnac, 225, 226, 227
M. Borelli, 266, 267, 269
M. Bouchu, 12

An INDEX *of* AUTHORS NAMES.

C

M. Carré, 10, 11, 319, 353, 354
M. Caſſet, 12
M. Caſſini, 25, 26, 85, 89, 93, 218, 223, 224, 226, 239, 251, 252, 253, 269, 305, 340, 374, 375, 386
M. Chazelles, 85
M. Clapiés, 218
M. Couplet, 85

D

M. Daleſme, 360, 361
M. Deliſle, 229, 230, 349, 350
M. Deliſle (apothecary) 219
M. Deſcartes, 10, 71, 301
Gerard de Dieppe, 244
M. Dieulamant, 389
M. Dodart, 7, 200

F

Chevalier de Fontenai, 270
F. Fulcheron, 374

G

Galileo, 390
M. Galland, 7
Abbé Galois, 351
Gaſſendi, 281
M. Geoffroy, 25, 72, 265, 267, 319, 352
M. de Glos, 109

F. Gouye, 21, 243
M. Grata, 225
M. Gualteri, 269
Guerick, 392

H

Dr. Halley, 64, 70, 240, 241, 242, 243, 244, 270, 271, 344, 350
M. du Hamel, 65, 351
Hartman, 229
M. des Hayes, 109
M. de la Hire, 5, 6, 12, 14, 43, 50, 65, 66, 70, 89, 90, 102, 116, 142, 144, 149, 201, 232, 236, 256, 273, 286, 305, 306, 307, 342, 343, 352, 362, 367, 390
M. Homberg, 15, 16, 218, 221, 222, 223, 224, 225, 312, 316, 347, 350, 351, 352
M. Hubin, 223, 224
M. Huygens, 103, 109, 111, 113, 202, 248, 393

I

M. Joblot, 10, 11
Iſidore, 293

L

F. Laval, 218, 399, 404, 400, 402
M. Leibnitz, 356, 357
M. Lemery, 219, 352, 353, 358
M. Lippi, 220, 221

M. Littre, 358, 359
Louera, 279

M

F. Mallebranche, 328
M. de Marchais, 349, 350
M. Maraldi 13, 14, 85, 137, 226, 304, 342, 347, 352, 374
M. Mariotte 52, 66, 79, 86, 89, 109, 111, 113, 114, 202, 246, 251, 252, 253, 254, 255, 256, 257, 260, 261, 262, 286, 287, 341, 342
F. Merſenne, 390
M. Mery, 17, 100, 119, 358
Moufet, 295

N

F. Noel, 243, 244, 272
M. Nuguet, 393, 397, 398

P

M. Parent, 3, 4, 5, 9, 38, 40, 42, 142, 143
M. Paſchal, 88, 251, 252, 254, 256, 391
M. Perier, 88, 251, 259, 260, 306, 341, 342
M. Petit, 390, 392
M. Picard, 87, 108, 110, 115, 202, 207, 277, 278, 307, 308, 309
M. de Plantade, 218, 223
Dr. Plot, 52
M. de Pontbriant, 236, 239, 367

M. Poupart, 161, 293, 353
M. Puget, 7

R

F. Riccioli, 243, 244
F. Richaud, 245
M. Richer, 108, 110
M. Roemer, 202
M. Rohaut, 10

S

Marquis Salvago, 375
Sanctorius, 45, 47, 391, 398
Sanſon, 230
M. Sarraſin 181, 184, 192, 197, 199
F. Sebaſtien, 304, 305, 306, 340, 341, 342
Don Franciſco de Seylas, 279
Swammerdam, 294

T

M. Thevenot, 202
M. du Torar, 236, 239, 367
Toricelli, 390, 391
M. Tournefort, 181
M. de Tournon, 239

V

M. Varin, 109
M. de Vauban, 238
M. de Vaubonnays, 12
M. du Verney, 94, 324, 125, 126, 127, 128, 129, 140
Vitruvius, 201
Iſaac Voſſius, 266, 267, 268.

ADDENDA.

THE following papers being ranged by M. *Fontenelle*, under the head of *aſtronomy*, were omitted in the preceding volumes; but as they conduce alſo to the improvement of geography, we have thought it proper to inſert them in this place.

In the MEMOIRS of 1701.

VII. *A compariſon of the principal phaſes of the eclipſe of the moon* Feb. 22, 1701, *obſerved in different towns of* Europe, *related to the academy* June 25, *by M.* Caſſini.

At Collioure.

The beginning could not be obſerved becauſe of the clouds, but by comparing together the phaſes of the ſame magnitude before and after the middle of the eclipſe,

	h	'	"
The middle of the eclipſe was found at	11	27	20
The total end was obſerved at	12	36	57
Therefore the half of the duration was	1	9	17
And the beginning of the eclipſe at	10	18	23

At Paris,

The clouds hindered the obſerving this eclipſe, but we have found by the *ſeries* of our triangles, that this city is more weſtward than *Paris* by 3′ of an hour: within about 1″, which we ſhall here neglect; having therefore ſubſtracted 3′ from the preceding phaſes, the beginning of the eclipſe muſt have been,

At *Paris* at	10	15	23
The middle at	11	24	40
The end at	12	33	57

We ſhall make uſe of theſe phaſes to find the difference of the meridians of the other places where this eclipſe has been obſerved.

At Madrid *by F.* Ulbôa *and* Caſſani, *jeſuits.*

				At Paris.			*Diff. of the merid.*	
	h	'	"	h	'	"	'	"
The beginning at	9	45	27	10	15	23	30	6
The end at	12	7	38	12	33	57	26	19
The duration at	2	22	21	2	18	34		
The half	1	11	10	1	9	17		
The middle	10	56	27	11	24	40	28	13

Theſe differences of the meridians are ſo different from one another, that we cannot depend upon them. That which is taken from the end, differs from that which was formerly obſerved from 23 to 24 minutes.

At Cordova, *by* D. Pedro Antonio de Blancas.

	h	'		
The beginning	9	45	30	23
The end	12	11	25	57
The duration	2	26		
The half	1	13		
The middle	10	58	26	40

The difference taken from the end ſeems to be preferable to that which is taken from the other phaſes.

At Pau, *by F.* Pallu, *a jeſuit.*

The beginning	10	4	11	23
The end	12	20	12	57
The duration	2	16		
The half	1	8		
The middle	11	12	12	40

At Avignon, *by F.* Bonſa, *a jeſuit.*

	h	′	″	*Diff. of the merid.* ′	″
The beginning	10	26	44	11	11
The end	12	43	28	9	31
The duration	2	16	44		
The half	1	8	22		
The middle	11	35	6	10	26

The difference taken from the end agrees well enough with the obſervations formerly made.

At Marſeilles, *by F.* Laval, *a jeſuit.*

The beginning	10	28	30	13	7
The end	12	45	40	11	43
The duration	2	17	10		
The half	1	8	35		
The middle	12	37	5	12	25
By the ſatellites of ♃				13	40
By the eclipſe of ☉ 1699				12	28

At Straſburg, *by M.* Eiſemſchmid.

The beginning	10	37	37	22	14
The end	12	56	45	22	48
The duration	2	19	8		
The half	1	9	34		
The middle	11	47	11	22	31
By the eclipſe of ☉ 1699				22	7
Difference of the meridians				21	21

At Nuremberg *by M.* Wurſzelbaur.

The beginn. a little doubtful	10	50	*	34	47
The end doubtful	13	7	36	33	39
The duration	2	17	36		
The half	1	8	48		
The middle	11	56	48	34	8
By the ſatellites of ♃				34	33

At Berlin, *by M.* Kirchius.

	h	′	″	*Diff. of the merid.* ′	″
The beginning	10	15	36	44	13
The end	13	18	40	44	43
The duration	2	19	4		
The half	1	9	32		
The middle	12	9	8	44	28
By the middle of *Tycho*				44	32

At Kiel *by M.* Rehyer.

	h	′		
The beginning	10	45		
The middle	12	0	35	20
The end	13	15		
The duration	2	30		

Magnitude 6¼

VIII. *A compariſon of ſeveral obſervations of the eclipſe of the ſun,* Sept. 23, 1699, *made in different towns of* Europe

At Paris, *in the royal obſervatory by* Meſſ. Caſſini *and* Maraldi.

	h	′	″
The beginning of the eclipſe at	8	15	0
The greateſt darkneſs of 9 dig. 29 min.	9	27	0
The end	10	45	5
The duration	2	30	5

At Chatenay, *by M. de* Maleſieu.

The beginning of the eclipſe at	8	15
End	10	47
Duration	2	32

The meridian of *Chatenay* is very near the meridian of the obſervatory to the weſtward.

At Marſeilles *by M.* Chazelles, *F.* Feuillée, *and* *F.* Laval.

	h	′	″
The beginning of the eclipſe obſerved at *Marſeilles*	8	30	3
By the figure at *Paris*	8	17	20
Difference of the meridians	0	12	43
The end of the eclipſe obſerved at *Marſeilles*	11	1	13
By the figure at *Paris*	10	49	0
Difference of the meridians		12	13
Mean between both		12	28

At Avignon, *by F.* Bonfa, *a jeſuit.*

The beginning of the eclipſe at *Avignon*	8	25	[illegible]
By the figure at *Paris*	8	16	40
Difference of the meridians		8	20
The end of the eclipſe at *Avignon*	10	57	17
By the figure at *Paris*	10	47	30
Difference of the meridians		9	47
Mean		9	3

At Arles, *by M.* Davizard.

The beginning of the eclipſe at *Arles*	8	24	30
By the figure at *Paris*	8	16	55
Difference of the meridians	0	7	35
The end of the eclipſe at *Arles*	10	53	30
By the figure at *Paris*	10	46	10
Difference of the meridians		7	20
Mean		7	27

At Rochelle, *by M.* des Hayes.

The beginning of the eclipſe at *Rochelle*	7	58	17
By the figure at *Paris*	8	13	0
Difference of the meridians		14	43

	h	′	″
The end of the eclipſe at *Rochelle*	10	23	7
By the figure at *Paris*	10	37	15
Difference of the meridians		14	8
Mean		14	26

At Genoa, *by the marquis* de Salvago *and the marquis* Alexander Grimaldi.

The eclipſe was already begun at *Genoa* at	8	45	7
The beginning of the eclipſe by the figure at *Paris*	8	19	40
Therefore the Difference of the meridians muſt be leſs than		25	27
The end of the eclipſe at *Genoa*	11	19	42
The end by the figure at *Paris*	10	55	35
Difference of the meridians		24	7

At Lyons, *by F.* S. Bonet, *a jeſuit.*

The eclipſe was already begun at *Lyons*	8	26	25
By the figure at *Paris*	8	16	0
Therefore the difference of the meridians muſt be leſs than		10	25
The end of the eclipſe at *Lyons*	10	57	20
By the figure at *Paris*	10	48	20
Difference of the meridians		9	0

At Madrid, *by the duke* d'Uzeda, *the* Abbé Scotti, *and* F. Kreza, *a jeſuit.*

The beginning was not obſerved at *Madrid*, becauſe of the bad weather.

The eclipſe at 1½ digit at *Madrid*	7	59	45
By the figure at *Paris*	8	23	0
Difference of the meridians	0	23	15
The end at *Madrid*	10	7	0
By the figure at *Paris*	10	29	10
Difference of the meridians		22	10

At Modena, *by F.* Fontana, *a Theatin.*

	h	'	"
The beginning at *Modena*	8	55	0
By the figure at *Paris*	8	20	45
Difference of the meridians	0	34	15
The end of the eclipse at *Modena*	11	33	20
By the figure at *Paris*	10	59	10
Difference of the meridians		34	10

At Strasburg, *by Meſſ.* Eiſenſchmid *and* Schmid.

The beginning of the eclipse at *Straſburg*	8	41	27
By the figure at *Paris*	8	19	20
Difference of the meridians		22	7
The end of the eclipse at *Strasburg*	11	15	21
By the figure at *Paris*	10	54	0
Difference of the meridians		21	21

At Bolonia, *by Meſſ.* Manfredi *and* Stancari.

The beginning at *Bolonia*	8	56	30
By the figure at *Paris*	8	21	20
Difference of the meridians		35	10
The end of the eclipse at *Bolonia*	11	36	49
By the figure at *Paris*	11	1	1
Difference of the meridians		35	49

At 12 *miles from* Parma *to the weſt by F.* Beccatelli, *a jeſuit.*

The beginning	8	55	38
By the figure at *Paris*	8	22	0
Difference of the meridians		33	38
The end of the eclipse	11	39	54
Perhaps	11	36	54
By the figure at *Paris*	11	2	20
Difference of the meridians		34	24

At Nuremburg, *by M.* Wurtzelbaur.

	h	'	"
The beginning of the eclipse	8	57	14
By the figure at *Paris*	8	22	55
Difference of the meridians		24	19
The end at *Nuremberg*	11	35	56
By the figure at *Paris*	10	59	30
Difference of the meridians		34	26
The mean		34	22

At Kiel, *by M.* Reiher.

The end of the eclipse	11	33	0
By the figure at *Paris*	10	37	15
Difference of the meridians		55	45

At Gripswald in Pomerania, *by M.* Pyle.

It was observed to be 2½ deg. eclipsed at	9	57	0
By the figure at *Paris*	8	44	15
Difference of the meridians		[illegible]	45
The end was observed at	11	42	30
By the figure at *Paris*	10	[illegible]	50
Difference of the meridians		[illegible]	40

IX. *The differences of the longitudes of several towns of France, taken from the observations of the eclipse of the sun of July 12, 1684.*

At Paris, *by M.* Cassini.

The beginning of the eclipse	1	22	[illegible]
The end	4	45	[illegible]

At Aix in Provence, *by M. le* Prieur Gautier.

The beginning at	1	24	30
The end	[illegible]	[illegible]	[illegible]

The

	h	'	"	
The hour of the end at the meridian at *Paris* by the figure	4	55	0	
Difference of the meridians		14	6	to the eaſt
Difference of longitude	3°	31	30	

At Avignon, *by F.* Bonſa.

The beginning	2	43	27	
The end	5	4	37	
The hour of the end at the meridian of *Paris* by the figure	4	56	3	
Difference of the meridians		8	34	to the eaſt
Difference of longitude	2°	8	30	

At Lyons, *by F.* Hoſte.

The end of the eclipſe	4	59	20	
The time of the end at the meridian of *Paris* by the figure	4	51	20	
Difference of the meridians		8	0	to the eaſt
Difference of longitude	2°	0	0	

At Honfleur, *by M.* de Glos, *profeſſor of hydrography.*

The beginning of the eclipſe	2	15	0	
The end	4	35	30	
The time of the end at the meridian of *Paris* by the figure	4	42	0	
Difference of the meridians		6	30	to the weſt
Difference of the longitude	1°	37	30	

At Pau, *by F.* Richaud.

The end of the eclipſe	3	45	0	
The time of the end at the meridian of *Paris* by the figure	4	54	0	
Difference of the meridians		9	0	to the weſt

Before Roſes, *by M.* Chazelles.

	h	'	"	
The beginning	2	40	0	
The end	5	1	30	
The time of the end at the meridian of *Paris* by the figure	4	57	30	
Difference of the meridians		4	0	to the eaſt
Difference of longitudes 1°.				

This laſt obſervation which ſhews *Roſes* to be more eaſtward than *Paris* by a degree ſhews, that the meridian of *Paris* does not go to *Perpignan*, as in the maps of *France*, but that it paſſes thro' the weſtern mountains of *Rouſſillon*.

This determination, which then ſeemed extraordinary, has been confirmed by the meridian of the obſervatory, being prolonged to theſe mountains in 1701.

M. *Maraldi* had uſed it in his map of *France*, publiſhed by the Sieur *Jaillot* in 1700. which is the firſt where the meridian of *Paris* has been directed thro' *France* to its true meridional bound in the *Pyreneans*.

FINIS.